THURSDAY, SEPTEMBER 29, 1870

HOUSE ACCOMMODATION FOR LEARNED SOCIETIES

THE movement which originated with the Statistical Society about three months ago for bringing under the shelter of one roof various learned societies of the metropolis, has already made that progress which gives the best assurance of ultimate success.

The "Learned Societies' Accommodation Committee" is at present constituted by delegates from the undermentioned bodies:—The Anthropological Society; the British Archæological Association; the East India Association; the Ethnological Society; the Institute of Actuaries; the Iron and Steel Institutes; the Juridical Society; the Meteorological Society; the Photographic Society; the Royal Colonial Institute; the Society of Arts; the Social Science Association and Law Amendment Society; the Statistical Society; and the Victoria Institute.

Each of these Societies is represented on the Committee by one delegate. The President of the Statistical Society, Mr. Newmarch, has been chosen chairman, but as this Society had already a delegate, the Chairman has no vote, otherwise the Society would have two voices at the Board, while the others were restricted to one each.

The Committee reserve to themselves the "power to add representatives from other learned Societies." By this resolution the combining societies may be increased, and probably will be, as the scheme approaches nearer accomplishment.

One body named in the list above has so large a fellowship, and so wide a scope in its objects, that its wants are consequently great and peculiar. The Society of Arts is likely to need house room in, we believe, a year or two. This society, Mr. Le Neve Foster remarks, would require "all the room we have at present and something more." When their extensive premises in the Adelphi are brought to mind, it is at once felt that Mr. Foster's society stands apart by the magnitude of its essential wants from all the others just named. To a certain degree the needs of the Society of Arts do not accord with the humbler demands of the other societies. The latter may be housed in a moderately capacious building, with a common meeting room or theatre, capable of accommodating from 150 to 200 members. Hence, these smaller scientific bodies offer a much easier undertaking to organise and manage. In London the difficulties of obtaining an appropriate site are enormously multiplied by any large increase of required frontage. On the other hand, the union of the Society of Arts with the other learned bodies presents the opportunity of a bolder enterprise. A comprehensive project for lodging all the London societies lacking house-room in one mansion is, doubtless, an attractive idea to many minds. Thus, it would appear at first sight, that, under these circumstances, two courses emerge: a moderate plan, with proximate execution, for the smaller societies; a grander scheme, with, in all probability, indefinitely remote accomplishment.

The Committee have avoided, by the unanimous resolutions of the 1st July, any conflict between these views. They have resolved in effect:

1. That convenient and permanent accommodation should be provided in a distinct building for societies that do not require extensive museums and libraries.

2. That the Committee express their earnest desire to co-operate with societies requiring larger accommodation for libraries and museums, either by "a combined application to Government for a site or building, or by joint action for the purchase of a convenient site."

The Committee think that if the wider co-operation spoken of in the second resolution should be successful, the plan for the smaller societies "may be either treated as a separate block in an associated group of buildings, or as a constituent part of one large building."

The Committee have taken the necessary step of giving instructions for the preparation by a competent architect of sketch plans and the elevation of a building adapted to the requirements of the smaller societies. These plans, it is understood, will be laid before the Committee when it re-assembles in October. Further, the Committee determined that the first subject for consideration at their next meeting "be the appointment of one or more of its members to represent its view and wishes to *The Aid to Science Commission*."

If, eventually, only the smaller scheme be adopted, it is thought the cost of the whole building and the purchase of the site could be compassed by an outlay which would offer no pecuniary impediment. In the absence, however, of working plans any stated sum can only be regarded as roughly approximate. The site itself may prove a business not so easily dealt with. Position is a matter of such precious importance to the utility of the undertaking that it is not likely to be undervalued—success or failure very much hangs upon the local habitation.

The proceedings of the committee seem to have been thus far, prompt and business-like. We have no doubt, therefore, that their efforts to economise the resources, and thereby augment the utility of the scientific societies of the metropolis, will speedily bear good fruit.

THE BERLIN WORKING MEN'S CLUB

FOR some years past there has been carried on at Berlin a Working Men's Club (for so it seems best to translate *Der Berliner Handwerker Verein*), established, we are told in its Reports, by working men and friends of labour, in order to promote general culture, sound technical knowledge, and good manners, among its members. This it attempts to do by means of popular lectures, classes for serious instruction, gymnastics, a library and reading-room well stocked with books and journals, concerts and social gatherings, or pleasure parties in which the wives and children of members take part. In many respects it resembles our own Mechanics' Institutes and Working Men's Clubs; but there are several features deserving special attention.

Not the least noteworthy point about it is its success. The average number of members at any one time is about 4,000, of whom nine-tenths at least are *bonâ fide* working men; but owing to the migratory habits of the German artizans, no less than 10,000 names are placed on their books every year. Well worthy of attention is the essentially democratic nature of the constitution and management. It is founded on the principle of self-help;

every man over 17 years of age, and unconvicted of crime, is eligible for membership, and after election continues to be a member so long as he pays his three silver groschen (somewhat less than 4*d.*) every month. The affairs are not conducted *for* the members by the philanthropic nobility, clergy, and gentry of the place, but *by* the members, through a representative assembly chosen annually, two-thirds at least of whom must be engaged in trade, and through a triennial committee, two-thirds of whom must also be engaged in trade. There is also a president, with vice-presidents, and a teacherhood, chosen either directly or indirectly by the members.

Yet in spite of, or rather perhaps because of, this working of self-help, they are enabled to enrol among their members, and receive active support from, the best and most active minds of Berlin. The leading professors of the University, the energetic young literary and scientific men of the city, the chief men of industry and commerce, all come forward to help them; and in their lists of popular lectures we find many names of European reputation.

The lectures, free to all members, and delivered in the club building, are intended to stir up the minds of the members to what is going on around them; while all those who are alert to the value of knowledge have access, on the payment of a small fee, to classes in which such subjects as reading, writing, arithmetic, geometry, mechanics, book-keeping, &c. &c., are taught in a thoroughly earnest and business-like manner. There are, besides, technical practical classes in construction and architecture; and the managers, having very definite views about play and work, take the greatest care that the musical and social gatherings shall be eminently successful in giving delight, and helpful in building up those "good manners" which are so desirable.

Then again, it is thoroughly catholic in spirit. It belongs to no religious or anti-religious party, advocates no political views, democratic, socialistic, or conservative; and, in fact, has no shibboleth of any kind whatever. It does not try to introduce serious learning and sound knowledge under false pretences. It does not call its members together to hear music, and then give them the stones of science. Every one can do as he likes. If he desire solid knowledge, there it is. If he care only for music or for a promenade, he can listen to the very best of the former, and walk about on fixed days among a crowd of comrades, all bent on pretending to be happy. He may, if he like, take out the whole of his monthly three groschen worth in abundant exercise on the parallel bars. So long as in any wholesome way the heavy burden of the artizan's daily life is lessened, and his dull life lighted up, the club thinks that it has in a measure accomplished its ends.

We imagine that most of our readers have read the article by Mr. James Stuart in No. 3 of NATURE, and they who have any experience of English workmen will, we venture to think, agree with nearly all that is there laid down. Putting together the burden of that article and the Report of this Berlin Working Men's Club, the question naturally suggests itself, Could we not in England do as much and a great deal more than is done in Berlin? It is one of the qualities of science, that he who has any is always anxious to give to others; and there surely could be no difficulty in establishing in London and in other large cities scientific teacherhoods willing to hold out the right hand of fellowship to all working men's clubs which were felt to be really acting on the principle of self-help.

There are many excellent and flourishing working men's clubs in London and elsewhere, and more than one working men's college. But we believe we are not going beyond the truth in stating that they lack the catholicity of the Berlin society. They are not all free from the suspicion of *arrière pensée* of some kind or other. Moreover, as separate institutions they tend to distract efforts, and cannot make a united appeal to the whole body of scientific men; while some of them at least are worked from above, the principle of dependence being more conspicuous than that of self-help. And so it comes to pass that those who are eager for knowledge have to struggle on without the counsel which they might otherwise share, often drawing their truth from wells by no means undefiled, and the dull unawakened multitude but rarely hear the voices which might rouse them from their sleep, and which now are wasted on the listless ears of fashionable audiences.

It is true that our scientific men are burdened far more than their German brethren with work undertaken for the sake of getting their daily bread, and for that alone; and so far unremunerative labour is to them a double task. But we have no doubt that, in spite of this, many would be led to come forward by the strong convictions they have that the welfare of their countrymen and their country is tottering for the lack of knowledge. By means of a little organisation, in which our artizans are pre-eminently skilled, much work might be got out of such teacherhoods, with the least possible wear and tear. They might deliver occasional lectures both for instruction and for rousing attention; but their chief use would be to advise the various clubs in their more serious work of class instruction, directing their studies, and assisting them in the selection of teachers.

Far be it from us to throw the slightest obstacle in the way of imperial secular education; but while waiting for the good time coming—in which the ladder of learning, from the lowest to the topmost rung, shall be free to every man, of every rank—much lies in the hands of the working men themselves. Let them show that they are ready for instruction, and have adopted the principle of self-help, and we believe that they will find scientific men ready to meet them half-way. It will not cost the workmen much to establish schools; it will not cost the teacherhood very much to give counsel; and the machinery for the payment of the actual class teachers is already in large measure provided for by the Science and Art Department of the Government.

The lamentable war that is now desolating some of the fairest provinces of Europe, and the unproductive current in which the energies of all Germans are at present conducted, must necessarily throw back this movement on the Continent. As long as we hear of peasant and professor working together in the field and on the ramparts, we cannot expect that they will also co-operate in the pursuit of scientific knowledge. But when this war-cloud has passed away, when France and Germany alike again turn to the arts of peace, it will be, we trust, with a renewed determination to persevere in that road which can alone lead to true national greatness.

Our own mechanics' institutes and kindred undertakings are just now commencing their autumnal session. We commend to them the consideration whether the principle of self-help cannot be more definitely recognised than has hitherto been the case. We hear with pleasure that the Working Men's College in Great Ormond Street is projecting an extension of its scientific programme in the approaching session. Let men and women be treated, not as artisans, mechanics, or gentlemen, but simply as men and women, standing toward the teacher only in the position of recipients of something which he feels the power and necessity of imparting; let no thought of any other relationship enter into this connection; and we predict for this and other equally admirable institutions a far wider popularity and usefulness than they have hitherto enjoyed.

REPLY TO PROFESSOR HUXLEY'S INAUGURAL ADDRESS AT LIVERPOOL ON THE QUESTION OF THE ORIGIN OF LIFE

II.

THE main argument must now be resumed: this having been only temporarily laid aside in order to inquire how far Prof. Huxley's "long chain of evidence" touched the real point at issue.

Having shown that, in reality, this has no immediate bearing upon the question in dispute, and having endeavoured to show to what extent the burden of proof rests with Prof. Huxley and others who affirm the universality of Redi's doctrine, it has now to be shown what evidence can be brought forward which may influence our judgment in the selection of one or other of the two possible modes by which alone the minute motionless specks of Living matter appearing in certain solutions can be supposed to originate. We must inquire, as much as possible independently of theoretical considerations, towards which of the two modes of origin—the germ or the germless—the evidence should induce us to lean.

It will be well, however, in the first place, to submit the following considerations to those who wish to form an unbiassed opinion upon the subject. Supposing that the minutest visible specks of living matter have originated from the growth of pre-existing *invisible* germs, there is still no reason whatever to induce us to believe that the invisible portions of Living matter would differ from visible portions in their power of resisting the destructive influence of heat. Whether visible or invisible, we are supposed only to have to do with Living matter, and it cannot be supposed that the qualities of this matter would vary simply *because* it existed in a state so minute as to elude our observation. What has been found to hold good, therefore, concerning the inability of visible Living matter to resist the destructive agency of heat may also be presumed to hold good for any invisible portions of Living matter. Invisible germs must be supposed to be amenable to the same influences as those which affect visible germs.* If the latter are destroyed by any given amount of heat, we should have every reason to expect that the former would also be destroyed under similar circumstances.†

It seems to me that *the only means* which we at present possess of throwing light upon this question, as to whether the minute Living things which appear under our eyes, in certain solutions, really derive their origin from pre-existing Living things, or spring into being *de novo*, is to subject other suitable solutions within hermetically sealed flasks, to a degree of heat which, on good evidence, is deemed adequate to kill all pre-existing Living things. If Living things are, notwithstanding the destructive exposure, subsequently to be found in the fluids when the flasks are opened, the evidence would seem to be strongly in favour of the *de novo* origination of such Living things—more especially if the heat employed had been great and long-continued. So far as all direct experiment and observation has hitherto gone, no Living thing whatsoever has been found to survive in a fluid which has been exposed for two or three minutes to a temperature of 110° C. And if we couple this fact with a due consideration of the fundamental unity in Nature of all Living matter, the supposition that any Living things—found in solutions that had been submitted to a *far greater* heat for two, three, or four hours—had braved this heat with impunity, would be an assumption seemingly much more improbable * than the only possible counter supposition, viz., that the Living things had been evolved *de novo*. The former supposition would be less likely to be true, because, instead of being consistent or harmonising with our general knowledge, it would seem to be a mere isolated fact bearing on its face the impress of grave improbability. *Bacteria* and fungus-spores which cannot, when made the subject of direct observation, resist the influence of a lower temperature, *are*, however, to be supposed capable of resisting the influence of a much higher temperature when their behaviour is watched by no human eye, though at a juncture when human prejudice emphatically requires that they should do so.† This extreme improbability—this isolated and otherwise unsupported notion—is cherished, whilst the other supposition, which is consistent with direct observation so far as it can go, and which is thoroughly in harmony with a great mass of scientific truth, is rejected. And why is it rejected? Because it is alleged that a great mass of human experience, having no immediate bearing upon this particular subject, and which is only related thereto by analogy, *seems* to make it improbable. And yet, as a matter of fact,—and although precisely the same reasoning is applicable against the alternative which they adopt—if the probability of a present *de novo* origination of Living things, after the fashion which is alone maintained, were to be admitted by every scientific man to-morrow, the whole body of human experience would remain perfectly undisturbed. A new probability, akin to a fact,‡ and one of the most extreme importance, would, it is true, have been added to the sum-total of human knowledge, and the only loss or contradiction would be, that those who had hitherto cherished the formula *omne vivum ex vivo* as the expression of a fundamental truth, would have to give it up. Like many another dogma, which in the course of time is toppled over, this expression of an over-hasty, though formerly justifiable, generalisation, now that it has been shown to be incompatible with the latest teachings of science, would have to fall into the shade of cold neglect.

* It was suggested to me by a friend that extreme smallness of size might be a protection against the influence of heat; in illustration of which possibility my attention was called to the fact that the water in capillary tubes will not freeze at times when that in larger vessels will become solidified. But although the water in the capillary tube does not freeze, this is due rather to some altered molecular condition of the fluid, and not because its temperature is not lowered just as much as that contained in the larger vessel which does freeze. I cannot see how smallness of size can confer immunity from alterations of temperature—more especially of any particles, however minute, which are contained within hermetically sealed flasks retained at a given heat for *four hours*.

† I have already pointed out (note p. 410) that the problem is utterly impossible to be solved if this be not granted as a probability; and that, similarly, without the concession that *invisible* crystalline matter resembled in its properties *visible* crystalline matter, it would be equally impossible to consider it as *proved* that a crystal can originate in a solution *de novo*, independently of a pre-existing crystalline germ.

* Although "germs," so far as we know them, are incapable of resisting the influence of great and prolonged heat, it was suggested by Prof. Rolleston, in the discussion which took place in the Biological Section on Sept. 21, that some germs *might* exist which were less amenable to the influence of heat, owing to the protein substances entering into their composition being in some peculiar isomeric state. We know for instance that *peptone*, which is a modification of albumen, is not coagulable by heat. All that we should deduce from this fact, however, seems to be this, that whereas ordinary albumen can, under the influence of heat, be made to undergo a certain isomeric modification by which it is rendered *insoluble*; this same albumen may, by a different process, be converted into *peptone*, a modification which is *not* capable of being converted into the *insoluble* isomeric condition by the application of heat. Too much stress must not be laid upon mere coagulability; and we must be, as it seems to me, further careful not to mix up our conception of this property too closely with another which is quite distinct, viz., as to the ability of Living things to withstand the influence of heat.

† Here we are brought face to face with the real difficulty. In order to explain the results of certain experiments, we *must* accept an apparent infraction of one or other of two rules which have hitherto been found to be universal, so far as human experience has gone. A Living thing has no more been known to be capable of surviving a temperatue of 150° C., than another Living thing has been *known* to arise *de novo*. Prof. Huxley, and those who think with him, appear to forget, in their present extreme unwillingness to give up the doctrine *omne vivum ex vivo*, that they can only retain it by abjuring another doctrine which has a similar seeming universality, so far as human experience has gone. We have nothing, then, but *probabilities* to guide us in our choice. Hence much difference of opinion will probably exist, till scientific men in general have come to adopt such physical doctrines concerning Life as those which Prof. Huxley has hitherto so ably taught.

‡ All so-called "facts" are, to the philosopher, only possibilities which vary in their degree of probability. This is inevitable, owing to the "Relativity of Knowledge," so that possibilities, probabilities, and facts, merge insensibly into one another.

What, then, are the facts which have been made known bearing upon the solution of this question?

Before the date of M. Pasteur's researches, it was generally supposed that Living things were incapable of surviving in a fluid which had been raised even for a few minutes to the temperature of 100° C.; but, after the results of his experiments, he claimed* to have a right to conclude therefrom that, whilst Living things were destroyed in acid fluids which had been raised for a few minutes to the temperature of 100° C., they were not certainly killed in alkaline fluids unless these had been raised for a few minutes to a temperature of 110° C.

This, however, is the point at which Prof. Huxley has chosen to close what he considers to be the history of the rise and progress of the doctrine expressed by the phrase *omne vivum ex vivo*. Then, ignoring all† that had been done in the interval between the years 1862 and 1870, he concludes a long but almost irrelevant chain of evidence with an account of three recent (?) experiments of his own, concerning the cogency and worth of which I have already spoken.

But let us briefly glance at the most important work which has been done, in order to throw light upon the subject in dispute, in the interval between the appearance of M. Pasteur's memoir in 1862 and the three experiments made by Prof. Huxley himself—work which he so summarily dismisses from notice.

I will say nothing now concerning the various experiments which have been made similar to those of M. Pasteur, but with contradictory results; I will refer rather to experiments in which the flasks and solutions employed have been exposed to a degree of *heat much higher and much more prolonged* than that which was proclaimed by M. Pasteur to be adequate to prevent the occurrence of all organisms in the solutions, and in which, nevertheless, Living things have been found on opening the flasks. As I have elsewhere mentioned,‡ Prof. Jeffries Wyman,§ of Cambridge, U.S., published an account in 1862 of experiments in which he had boiled fluids containing organic matter for a period of two hours, under a pressure of two atmospheres, that is to say, at a temperature of 120°·6 C. To the fluid so treated, no air was allowed access except what had passed through the capillary bores of white-hot iron tubes. And yet, when, after a certain time, the flasks were broken, Living organisms were found in the fluids contained therein. Prof. Mantegazza,‖ of Turin, has obtained Living organisms from the fluids of hermetically closed flasks after these, containing the putrescible fluids and common air at ordinary atmospheric pressure, had been subjected for some time to a temperature of 140° C. Prof. Cantoni,** of Pavia, has found Living bacteria and vibrios in the fluids of similarly-prepared closed flasks, after these had been exposed in a Pepin's digester to a temperature of 142° C. for four hours. And, lastly, I have myself recorded experiments,†† made with the kind assistance of Prof. Frankland, showing that Living organisms almost similar to those which have been ascertained to be incapable of resisting the influence of a fluid raised to the temperature of 100° C. for a few minutes may be met with, after a time, in solutions which had been exposed in hermetically-sealed and airless flasks, to a temperature varying between 146° and 153° C. for a period of four hours. Whilst, by another experiment,‡‡ it was found that a fungus and spores, as nearly as possible similar to that which had been found in a living state in one of the former experiments, were all *completely disintegrated*,§§ after exposure for an equal period, and in a flask containing a similar solution, to the same temperature of 146° to 153° C.

Now, in reference to these results, it should be remarked that there is not one tittle of evidence, so far as I am aware, which can be adduced tending to show that any single Living thing can continue to *live* in a fluid which is exposed even for a few minutes to a temperature of 110° C.—the degree of heat which M. Pasteur thought necessary to ensure the destruction of all pre-existing Living things. And also it has been shown just as definitely that none of the lower Living things which have been submitted to the test, have ever been found to survive an exposure in *dry air** for 30′ to a temperature of 130° C. Still less, therefore, would they be capable of withstanding the influence of an *extremely condensed vapour* at a temperature of 150° C., or even at 140° C., for a period of *four hours*.† There is, at present, no reason whatever for inducing people to believe that the living things met with in the experiments of Professors Wyman, Mantegazza, Cantoni, and those made by myself in concert with Prof. Frankland, had been derived from germs which were capable of living through the fiery ordeal to which the flasks had been submitted, save the extreme reluctance of these people to bring themselves to believe that Livings things can now‡ arise independently of pre-existing Living matter. Moreover, it should be understood, that experiments of this kind seem to be such as are alone capable of aiding us to come to a conclusion on this, the only question in dispute—whether the motionless specks which appear in previously homogeneous solutions, are more likely to have proceeded from the growth of pre-existing *invisible* germs, or to have arisen quite independently of pre-existing Living matter, under the influence of molecular affinities analogous to those which are believed to lead to the formation of similar specks of crystalline matter.

And yet, without one word concerning the limits of vital resistance; with what must be considered as a tacit admission that the very organisms in question are destroyed in a fluid maintained at a temperature of 100° C., for 15 minutes; without a single explicit mention of the experiments to which I have just been referring; with a seeming utter inappreciation of their important bearing upon the great question at issue—Prof. Huxley, closing his historical summary with a notice of the labours of M. Pasteur, ends an almost completely irrelevant statement with the mention of three experiments of his own, which, if they are not to be considered as altogether worthless, are, certainly, of no conceivable value for the establishment of the doctrine which he supports, or for the overthrow of the supposition that Living things can at the present time arise *de novo*.

Surveying the field of science from the elevated "position in which the suffrages of his colleagues had, for the time, placed him," recognising it as one of his privileges and duties, with "due impartiality," to declare "where the advanced posts of science had been driven in, or a long-continued siege had made *no* progress," Prof. Huxley ventures, in the face of the facts above-

* That M. Pasteur's experiments did not warrant him, however, in coming to the conclusion that Living things were capable of living in an alkaline solution when this was exposed to a temperature of 100° C., I have endeavoured to show in NATURE, No. 37, pp. 224—228.

† With the exception of the recent investigations of Prof. Tyndall, which Prof. Huxley considers capable of supporting his own view of the question, although Prof. Tyndall has really done nothing whatever to convince the public that the organic dust which exists in the atmosphere is even in part made up of the "germs," about which he talks so freely.

‡ NATURE, No. 35, p. 175.

§ "*Experiments on the Formation of Infusoria, &c.*" Cambridge, U.S.

‖ These experiments were not made in the interval above referred to, but even ten years before the publication of M. Pasteur's memoir. See *Journal de l'Institut, Lombard*, t. iii., 1852.

** See Gaz. Med. Ital. Lombard. Ser. Der. v. t. i, 1868, and two communications made to the Royal Lombard Institute, one in April 1868, and one in November 1869.

†† NATURE, No. 36. ‡‡ Ibid.

§§ See NATURE, No. 37, p. 219. The experiment in which the somewhat similar fungus was met with was No. 19 (NATURE, No. 36, p. 200), and to this I would particularly direct the reader's attention. The mode of appearance of the fungus, its gradual increase in size, as well as its microscopical characters, all point to its having been *a living fungus*. Whilst the partial preservation of the vacuum for 65 days shows pretty plainly that there was no unobserved crack in the glass. The partial destruction of the vacuum was most probably *due to the liberation of gases within the flask*, owing to some decomposition of the tartrate of ammonia during the growth of the fungus. *It is not likely that germs contained in the air could get through a crack, if any such existed, which was impervious to the air itself.*

* NATURE, No. 35, p. 170.

† Prof. Tyndall seemed to have completely forgotten all this during the discussion which took place in the Biological Section of the British Association on Wednesday, September 21. He there alleged as his principal reason why the conclusions which I am inclined to draw from my experiments should not be drawn—after I had pointed out to him that *I had no wish to exclude "germs" or Living things* from the flasks which were hermetically sealed,—that germs might have adhered to the upper portion of the flask, and might never have come into contact with the heated fluid. But this objection was seen to be futile in the face of the work which had been done concerning the influence of *dry heated air* upon lower kinds of Living things—work of whose existence Prof. Tyndall seemed to be in ignorance, or which he had entirely forgotten, until he was reminded of the opinions of M. Pasteur on this subject. Prof. Tyndall, indeed, *seemed* to know very little more of M. Pasteur's views than he did of my own. Until it can be shown, however, that any single minute Living thing *can* withstand the influence of a *condensed vapour* at 150° C., for four hours, the objection which he started so triumphantly remains and exists only as a *highly improbable supposition*, in the face of which I can again fearlessly state my conclusion—that, taking all the *evidence* as it at present exists, I am as much, even more, entitled to believe that the organisms found in my flasks had been evolved *de novo*, than that they had been produced from pre-existing germs of Living matter, seeing how universally destructible this has been shown to be by heat.

‡ Even though some of these are quite willing to admit the *possibilty* of such an occurrence, and are ready to accept the notion that in past ages of the earth the first Living matter did so originate from a combination of mere non-living materials.

mentioned, to tell the British Association for the Advancement of Science, and the public generally, that Redi's great doctrine appears to be "victorious along the whole line;" whilst the views and experiments of those who think differently are thus referred to:—"On the other side the sole assertions worthy of attention are that hermetically sealed fluids which have been exposed to long-continued heat have sometimes exhibited Living forms of low organisation when they have been opened."

All comments on such a proceeding seem needless—the facts speak only too plainly for themselves.

I will, however, say a few words concerning the mere empty generalities which Prof. Huxley opposes to the definite results of an honest band of workers.

He commences in this way:—"The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions, are preserved to the extent, I suppose I may say, of thousands of tons every year by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well *boiled* in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy." This is a very plausible statement, certainly; and one apparently tending to confirm Prof. Huxley's views. But what are the real facts of the case? I have made many inquiries and some microscopical examinations during the last three days, the results of which I will now communicate to Prof. Huxley and others.

Having visited one of the largest establishments in London, and seen the whole process to which the meats and vegetables are submitted for preservation, the information I have to convey is of the most authentic description. For this opportunity, and for many particulars communicated in a long conversation, I am much indebted to the courtesy of Mr. McCall, of Houndsditch.*

A number of cases, enclosing the provisions, instead of being simply heated to a temperature of 212° F. as most people would understand from what Prof. Huxley said, are first heated in a large chloride of calcium bath (warmed by steam) to a temperature of 230° to 235° F. for more than an hour and a half. The hole through which the steam has been issuing is then closed with solder, and as soon as the last of the set has been thus hermetically sealed, a higher pressure of steam is turned on, by which the bath is quickly raised to a temperature of from 258° to 260° C.†—at which temperature it is maintained for more than half an hour. Thus it is now learned that the meats are exposed to a heat of 230° to 235° F. for more than one and a half hours, and then to a temperature of 258° to 260° F. for another half hour at least. All this is very different from the simple statement that the provisions are "boiled," for a time not specified. Prof. Huxley, in the next place, mentions the possibility of failure, though he seems to attribute all these to "unskilfully closed tins." Now, on inquiry, it appears that the number of unmistakable failures even in the very best establishments is very appreciable, and although many of these failures may be accounted for by defective closure, Mr. McCall assured me that in a certain number of cases, where not the smallest defect could be detected in the tin, where the mode of preparation was unexceptionable, and the provisions originally of the best description, yet for some inscrutable reason some of these tins did prove utter failures. Gas was found to be evolved within, causing them to bulge at the extremities, and when opened the meats either showed a central decomposition of a most fœtid character without mould, or else mould might be found on some portions of the surface. He further assured me that certain tins which had been thoroughly well prepared, and in which the provisions seemed to remain in a perfect state of preservation‡ for two or even three years, might more or less suddenly show signs of a considerable evolution of gas within, owing to the provisions having fallen into a state of putrefaction. In other instances provisions would keep for ten years or more without any appreciable change. I was informed also that turtle, and all the soups which solidified when cold, invariably remained good. Amongst these there were no failures. Mr. McCall was somewhat doubtful as to whether in hot weather, provisions were more prone to fail after severe thunder storms. He had, however, "often thought that electricity" had something to do with the failures. Some of the large retail sellers spoke much more decidedly to me as to the number of failures after thunder. On this question, however, I lay no stress—I merely repeat what I was told.

Wishing to learn what microscopical appearances would be presented by provisions which were sold as being "perfectly good," I procured three specimens from two of the most esteemed retail establishments, informing the original owners that I wished to submit them to a microscopical examination. One of these was a tin of "Julienne Soup," which had been prepared ten months; the second, "Salmon," prepared six months; and the third, "Lobster," only six weeks old. The "Salmon" when opened, had not an altogether pleasant smell; the other two seemed quite fresh. In portions taken from the surface of each, I found the most unmistakable evidences that slight changes had taken place. All presented an abundance of flat granular aggregations,* figure-of-8 bodies, and a very appreciable quantity of *Bacteria* and *Leptothrix* filaments—some of these latter being plain and others jointed. The *Leptothrix* filaments were mostly about $\frac{1}{30000}$" in diameter. Some of the *Bacteria* were $\frac{1}{6000}$" in length, and many were moving pretty actively in the specimens taken from the "Julienne" and the "Lobster" figure-of-8 particles. In the "Salmon," I also found, during my comparatively short examination, two or three portions of *Fungus*-filaments, having dissepiments within, and measuring $\frac{1}{6000}$" in diameter.

Thus, to sum up, it appears that provisions, prepared as above described,† which have been exposed for more than two hours and twenty minutes to a temperature varying from 110° to 126° C. do, not unfrequently, *for no discoverable reason*, fall into a state of decomposition which renders them useless, and that the only specimens which I have examined microscopically, three in number, all presented evidences that Living things had been growing and developing in the hermetically sealed tins.‡ Why, in some cases, the changes should be so small in extent as not to impair the value of the provisions, and in other cases these changes—passing through the more intermediate grades—should render the provisions utterly useless, I, or others holding similar opinions, can scarcely be called upon fully to explain. Certain it is, however, that the facts above mentioned, including the circumstance that the failures sometimes take place after the tins have been hermetically sealed for two or three years, and that gelatinous§ substances are the least prone to change—are all

* And also for the kind permission to make known what he had told me.

† Whilst I was in this establishment one of the baths was seen to have reached a temperature of 263°. F. It was boiling very briskly The more or less solid contents of the tins would require a longer time to be raised to any given temperature than a fluid; so that, practically, the meats may have been exposed only for a comparatively short period to the higher temperatures mentioned. I may state that 230° and 260° Fahr., correspond to 110° and 126·6° C.

‡ As judged by evidences of a vacuum within.

* Some of these had undoubtedly arisen from a granular degeneration of the meats themselves. Some muscular fibres presented a healthy appearance, while others were more or less completely granular.

† I may state in reply to what was said by Mr. Eddowes in the discussion on Sept. 21, that the provisions examined by me had all been prepared by a process essentially similar to that adopted by Mr. McCall. I took care to ascertain this. The "salmon" was not prepared, as he supposed, in Canada, but by a well-known Scotch house.

Since the above was written, I have (Sept. 26) examined two tins which were prepared by Mr. McCall in 1861. One containing "Lamb and Vegetables" was perfectly good. It contained not a drop of fluid, though some glutinous matter was present. On microscopical examination I could find *no trace* of organisms. The other tin, containing "Veal and Peas," was also perfectly good; the odour was just like that of fresh meat. The contents were very dry, not a drop of fluid could be procured, although the surface was bedewed with a slight moisture. When a small portion, scraped from the surface and mixed with a drop of water, was examined microscopically, hundreds of extremely minute *Bacteria* and monilated chains were seen—all either dead, or else extremely languid. These results are very interesting when compared with what was found in the three other tins, whose contents were much more moist and contained actual fluid.

‡ Without reference to the question whether the *Bacteria* and *Leptothrix* filaments were living when seen by me, the very fact of their having been formed in such a very appreciable quantity, seems to make it more probable that they had been *developed* after the exposure to the heat within the hermetically closed tins, than that they had pre-existed in the fresh provisions in the state in which they were found. There was, however, no reason whatever for supposing that the *Leptothrix* filaments were dead, or that the slow movements of the *Bacteria* were not *languid vital* movements; between which and Brownian movements it is impossible to draw any line of demarcation.

§ It could not be supposed that a gelatinous substance would afford facilities for the molecular rearrangements to take place, without which no new evolution of Living matter would seem possible. On the other hand, if the Living things which are sometimes found in these cases are derived, as many will suppose, from undestroyed germs, it does not seem so easy to understand why they should not germinate on the surface of a gelatinous substance. The "Julienne soup" examined was not gelatinous, it rather resembled a moderately thick solution of gum in consistence.

rather strongly in favour of my view of the case, and will continue to be so, so long as our *knowledge* concerning the inability of Living things to resist the destructive influence of very high temperatures remains in anything like the same condition as it is at the present day.

Prof. Huxley is inclined to believe that there has been some error about the experiments recorded by myself and others. With regard to my own experiments, however, the chances of error were certainly diminished to a minimum. Certain fluids were placed in glass vessels, and were handed over to one of the most accomplished chemists in this country, with the simple request that he would extract most of the atmospheric air from the flasks, would seal them hermetically, and would then expose them to a temperature of 150° C. for four hours. All this is certified by Prof. Frankland to have been faithfully done.* One of the flasks was opened in the presence of Prof. Huxley himself, whilst another of them was opened in the presence of Prof. Sharpey; and although the others were opened when I was alone, I hope the results are none the less reliable. In the face of these facts, and of what has been detailed elsewhere, it seems difficult to imagine that the experiments are not really trustworthy.†

Prof. Huxley then concludes his observations on these experiments by saying:—"But if, in the present state of science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, *for no valid or intelligible reason that is assigned*, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be even for a moment doubtful."

Although this climax is thoroughly consistent with the style of the preceding remarks, I find it very difficult to understand why Prof. Huxley should have so much departed from his usual method of argumentation. I should like to ask him, however, whether he considers it the function of a scientific investigator to believe *only* in such seeming possibilities as he can at the time explain or account for; and also whether he who believes in the analogy between crystals and organisms,‡ can "assign any valid or intelligible reason" which is likely to be satisfactory to himself or to others, why the constituents of common salt, when in solution, should under certain circumstances aggregate into crystals of a cubical form; and why, on the other hand, the constituents of sulphate of soda should aggregate into rhombic crystals. Notwithstanding his inability to explain these facts, I suppose he nevertheless accepts them *as facts*, even although in the case of sulphate of soda, *almost exactly the same kinds of crystals result, whether they have proceeded from pre-existing crystalline germs, or whether they have arisen de novo.*§ Prof. Huxley seems only too much to overlook the fact that what may be perfectly inexplicable from one point of view, may, on the contrary, flow as a necessary consequence from one of an opposite nature. Although, therefore, as a disciple of Redi, the facts to which he has alluded may seem difficult to explain, Prof. Huxley must recollect that two rival doctrines are in question. And having two doctrines of almost equal probability to decide between, it seems to me mere childishness to reject a certain well-supported interpretation simply because it is inexplicable on the one hypothesis, and to think that this inexplicability is an argument against the interpretation given, when, so far from being inexplicable, this, *in the light of the counter hypothesis*, is nothing else than a logical consequence. That some such similarity as that which is alluded to should exist, is only to be expected by those who believe that the lowest living things are but the products of the molecular properties of a complex matter, and the "conditions" acting thereupon.* I entirely agree with Mr. G. H. Lewes, when, in a most valuable essay,† he points out that "similarity in the laws and conditions of Organic Combination must produce similarity in organisms, independently of relationship, just as similarity in the laws and conditions of inorganic combination will produce identity in chemical species." It is the extreme complexity of the materials in the one case, and their corresponding sensitiveness to modifying influences, which make it hopeless for us to think of ever getting the same uniformity of results, which we *are* able to attain when we have to do with simple inorganic materials. The difference, however, is one of degree, not of kind.

I enter a protest, therefore, against the first portion of Prof. Huxley's Inaugural Address, for the following reasons:—

1. Because it does not seem to be characterised by "due impartiality."

2. Because it is calculated to mislead the public; since what is represented as relevant and of first importance, has only an indirect bearing on the subject: *Abundance or paucity of germs in atmosphere.*

3. Because the real issues having already been pointed out by others, Prof. Huxley ignoring these, approaches the problem as though they had never been stated, and as though he himself were not aware of them: *Mode of origin of specks of Living matter in apparently homogeneous solutions.*

4. Because it allows room for the inference, and even suggests it, that evidence which is generally admitted to be of the greatest importance for the solution of the question in dispute, is really of little or no importance: *Limits of vital resistance to heat, and presence of Living organisms in closed vessels which had been previously exposed to great heat.*

5. Because, without any sufficient warrant, it throws doubt upon the "trustworthiness" of certain experiments, of whose real nature his audience and the public are not informed: *Experiments of Wyman, Mantegazza, Cantoni, &c.*

6. Because it opposes the definite results of these experiments by nothing but insufficient statements, and what appear to be crude suppositions: *Statements and assumptions concerning preserved meats.*

The general effect being, I conceive, an entire misrepresentation of the present state of knowledge upon the questions concerning the Origin of Life, which are at present under discussion.

H. CHARLTON BASTIAN

* See his description of the process, NATURE, No. 36, p. 199.

† The possibilities of error, which in a previous discussion (on Sept. 20) in the Biological Section, seem to have been suggested by Prof. Huxley, were two in number. First, that unperceived cracks may have been present in the hermetically sealed flasks, and second, that objects supposed to have been *Living*, may not have been so in reality. I have already spoken of these possibilities with reference to *Exp.* 19, and there is no better ground for either of the suppositions in reference to *Exps.* 17, 18, and 20. (See NATURE, No. 36, pp. 199—201.)

‡ See quotation, NATURE, No. 46, p. 411.

§ There is a very slight difference in the form of the crystals in the two cases, because in order to make sure of the absence of crystalline germs, the new crystals have to form under a different and exceptional set of conditions. But, notwithstanding what Prof. Huxley says, we find even a more striking divergence occasionally, in the case of organisms, which possibly have been evolved from similar materials though under different conditions. I have elsewhere said (NATURE, No. 37, p. 223):—"We find also associated with different sets of conditions, different kinds of Living things. In none of the crystals of tartrate of ammonia have I ever found a single distinct bacterium, and there has been the same complete absence of organisms of this kind in all my experimental fluids containing tartrate of ammonia and phosphate of soda, which have been sealed up *in vacuo*. This agreement is very striking, seeing that whenever a similar fluid, or a solution of tartrate of ammonia alone, is exposed to the air, then bacteria appear in abundance. There is a marked accordance, then, between the organisms which are produced in the experimental tubes *in vacuo*, and those which come from the cavities within the crystals," whilst these differ altogether from those which are met with in a similar solution exposed to the air. (See also what is said in *Note* on same page concerning the occurrence of *Sarcina*.)

* It is difficult, almost impossible, for us to say how far seemingly great differences in conditions, are really very different in respect to the influences which are most potential in leading the not-living to assume Living modes of combination, because we do not know for certain what these *most* potential factors are, and therefore how far these may be present or absent under circumstances apparently dissimilar.

† "Darwin's Hypothesis," *Fortnightly Review*, April 1868, p. 372.

*** Owing to the great pressure on our space, we are compelled to postpone several articles of real value which are already in type.*

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

University College Lectures for Ladies

IN this week's number of NATURE I see it noted that among the courses of lectures announced for the ensuing winter by the Ladies' Educational Association in connection with University College, are included two upon scientific subjects (chemistry and experimental physics). May I venture to point out that the prospectus makes mention also of a third, namely, on logic, introduced by ten lectures on the psychology of intellect? This course

was, certainly, designed as a scientific one, and indeed as a substitute for the course on mathematics suspended temporarily by force of circumstances. My own motive for drawing attention to the point will not, I think, be misunderstood, when so lately I had occasion in your columns to say a word for psychology as a natural science.

University College, Sept. 23 G. CROOM ROBERTSON

Mirage

IN connection with Mr. Kingsley's letter in your number of today on Mirage, I may mention that when in a steamer going up the Throndhjem Fiord, in Norway, last July, I saw some remarkable Mirage effects. In one case there appeared to be a large city, which altered as the ship advanced into a long line of very white cliffs of basaltic formation and then disappeared, and nothing was seen but very some low rocks; in other cases there appeared to be rocks suspended in the air at some distance from the surface of the water. It was a fine afternoon, and the sea very calm. W. P. M.

Liverpool, Sept. 22

Meteor

AT 8.30 p.m. on Sunday the 11th, a fine meteor was seen in the zenith traversing from East to West. It had a comet-like tail, and a star-like head; visible altogether for about ten seconds. In passing there was a "hish" sound, as of a rocket.

At 8 p.m. on Thursday the 15th, the Aurora or Northern Lights were very bright—mostly red, divided by rays of whiter light. Many persons, who were upon the pier, thought there was "a fire somewhere!"

Lowestoft, Sept. 16 SEPTIMUS PIESSE

Origin of Species and of Languages

THE extreme brevity of my former letter on this subject seems to have hindered Mr. Ransom, and perhaps other readers, from appreciating the analogical argument I used. Will you, in consideration of the importance of the inquiry, allow me now to illustrate that argument at greater length?

There are two sets of facts that stand out in marked contrast. No irrational animal has ever formed a language. Man alone, in all his varieties, has.

I agree with Mr. Ransom that no language has originated from an *intention* to form a new language; I see no reason to doubt that languages have arisen from the gradual variation, selection, and combination of a few primary sounds; and I think that existing languages are constantly undergoing change through the operation of physical, physiological, and other natural causes, irrespectively of reason. But the fact remains to be accounted for, that no animal unendowed with reason has ever selected and combined sounds into a language.

The cause does not lie in a want of significant sounds to begin with. No one who has ever owned a dog is ignorant how many emotional sounds—sounds, too, that vary greatly in individuals and varieties—*he* makes use of; but he has never even begun to make a language of them. Neither does the cause lie in a want of power to distinguish, and in the case of some animals, to imitate very accurately the natural sounds they hear, so as to have a supply of vocal symbols for things and occurrences ready for adoption if they will. But can any irrational animal be named that has ever begun to use such sounds as symbols denoting things or events, still less to modify them in order to express modified meanings, and far less to combine them into symbols of complex things, or into phrases, propositions, and sentences? The mocking-bird mimics the song of the whip-poor-will, the creaking of the wheelbarrow, the lowing of the ox, and the pattering of the rain; but does it ever, like the Greeks, Romans, and Gaels, speak of the ox by the name of *bo;* or, like us, speak of the rain as *pattering:* or modify that sound, like the Hebrew and the Teutonic races, into a name for the substance that patters (*matar, water, Wasser*), and use it to tell that it wants a drink? Least of all, has any irrational animal ever juxta-placed sounds, as the Chinese do, in different orders to express different relations between the things they denote; or with Aryans, modified sounds into prefixes and terminations to express metaphorically such abstract relations?

Every step in these processes involves an exercise of reason. True, there is no grand intention on the part of one man or nation to form a language, but there are countless intentions of individual men to express individual ideas and thoughts as they emerge, or to express them more accurately than before; and then, when one man by an exercise of reason devises and uses a new symbol or phrase, others imitate and adopt it. And so, while I admit that there are unintentional variations of words, and consequently (by degrees) of languages; and while I admit that there has been no intention to form a language as a whole, I think we must say that it is by countless intentions of rational beings that languages have been gradually formed.

It may be objected that savages possess languages, and that *they* are not rational. "My monkey *Wallady*," writes Sir Samuel Baker, "looks like a civilised being in comparison with the Nuehr savages." And yet, while the Neuhr savages have a language, Wallady has none, any more than my terrier Shag, knowing fellow though he be. Why this contrast, but because the most savage man is differenced from all other animals by the possession of reason?

Now, then, the argument against the theory of the formation of the species, or of their endowment with new organs, by a reasonless process, is this:—The experiment of the possibility of such a thing has been actually tried on the most extensive scale in the analogous matter of language, *and has failed*—failed in every instance except where reason has been at work to prompt and direct. Ought we not then to pause, while our data are so imperfect, and while science is making strides that may soon bring her to a point of view that will show her present logic to have been utterly at fault—to pause before entertaining a thought so revolutionary and perilous as that an eye, a beast, a man has been formed without presiding intelligence or design at all.

The subject is seductive; but I fear I have already encroached too much on your space. WILLIAM TAYLOR

The Cockroach

THE cockroach (*Blatta orientalis, Linn.*) has found an apologist in Dr. Norman Macleod, who asserts his incredulity in the current stories of this insect's bad habits. Cockroaches look, he says, like black priests among the beetles, and, like the priesthood generally, have been made the objects of misrepresentation and slander. Anyhow, the doctor treats as mythical the tradition, constant on ship-board, that cockroaches are in the habit of nibbling the nails of those who sleep with their feet uncovered. Not only are they harmless, but they are absolutely useful, inasmuch as they may be readily trod upon and killed by all who are willing to gratify their feelings of disgust and benefit society. In the history of the cockroach we can trace the origin of the nail-nibbling myth, if myth it be. The insect is indigenous in the warmer parts of America, and, in spite of its Linnean name, is only oriental through having been carried to the East by shipping. It has a natural love for warmth and for sweet things, and can indulge the latter taste by feasting on the feet of natives engaged in sugar manufacture. If Gilbert White is correct in his surmise that the insect was not introduced into England until late in the last century, its powers of reproduction and adaptation must be very large. It is, of course, very difficult to identify with absolute certainty the insects mentioned in classical authors, but there is a good deal to lead one to suppose that the μυλακρὶς mentioned by Aristotle and the *Blatta histrionorum* of Latin writers was the same as our loathsome pest. The English name is curious and worth investigation, but unhappily there is so much guess-work employed in derivations that this branch of philology cannot claim to be recognised as one of the "exact sciences."

Norton Court, Weobley C. J. ROBINSON

On the Dissipation of Energy

THE value of the successive numbers of NATURE is not a little enhanced by the papers of Professor Balfour Stewart on "Energy," which also lead us to long for his forthcoming volume on "Physics." If that work prove equal to that which he has already published on "Heat," it will give us a manual which may well compare with the best of those which have been published abroad, and it will besides possess a freshness of its own.

But is it desirable that the doctrine of the conservation of energy should be represented everywhere as a modern discovery? No doubt *the experimental verification* of the transformability of equivalent quantities of mechanical power of various kinds into equivalent temperatures is a modern discovery. But *the doctrine itself*

belongs to the epoch of Leibnitz, and was deliberately set forth by that great philosopher himself. "I do not" (says he) "undertake [in his last correspondence with Dr. S. Clarke] to establish my dynamics or my doctrine of forces. This would not be the proper place for doing so. However, I can very well reply here to the objection that has been made to me. *I have maintained the conservation of active forces in the world.* It was objected to this, that two soft non-elastic bodies on meeting lose some of their force. I answer no. It is true that the masses lose it as to their entire movement, but their particles receive it, being internally agitated by the force of the meeting. Thus the loss is only in appearance. The forces are not lost but only dissipated among the small particles. Now this is not to be lost, but to act as those do who change a piece of money into small coin." * Is it said that in the above words Leibnitz does not state that one of the principal forms of the incident force when dissipated in bodies is heat, which is known now to be the fact, or that perhaps he did not know that heat was a mode of action at all? the answer is, that it was never doubted by any philosopher of that epoch that heat is a mode of motion. And in the *Micrographia* of R. Hooke (see specially Obs. 6, 7, and 8), there is a discussion of phenomena from this point of view as interesting and as accurate as any that is to be found in any modern book.

But a no less important consideration for science is the decision of Sir W. Thomson, Professor B. Stewart, and other physicists of this school, that the doctrine of the dissipation of energy is co-ordinate with its conservation, and that the destiny of the universe is to become its own cemetery! This theory, expressed in its most general terms is to the effect that all rectilinear motions (locomotion) naturally tend to be transformed into intimately reciprocating motions (heat) which are naturally irrecoverable into their first forms, and which tend to be ultimately so distributed that externally universal repose or death must ensue. Now, in view of all the experiments that have as yet been made, and all the results of equivalent transformations between foot-pounds and temperatures which have been obtained, this is surely a very bold generalisation. It is an inference from what man finds in his *work* as to what nature must do in her *play*. But there are differences between the two which have not been duly considered. Thus to us the concrete state is everything, for we are ourselves concretes. We live from moment to moment only by condensing and concreting the aeriform. But Nature delights in the aeriform. And such is the tendency of material elements in general to the aeriform state, as to lead legitimately to the inference that the concrete state is a forced and a defective state of matter which is possible to the material elements only when somewhat of their full complement of virtue has gone out of them. As to heat, has not the production of heat in a concrete body expressly for its function to emancipate the constituent particles more or less, so as to put them in the way of gaining the aeriform state? Are not almost all concretes at almost all known temperatures continually giving off particles into the aeriform state, and that all the more the hotter they are? Even iron and clay are not without a smell in a damp atmosphere. Adopting then, as is now generally done, the aeriform or nebular state as primeval, is not the proper corollary of the doctrine of the conservation of force—not the reduction of the system to a dead mass, but its restoration to a fully aeriform nebular or ætherial state, with new fitness for producing all those phenomena which the nebular hypothesis supposes? Would not such be the issue supposing the cosmical action to be all in one direction? And if that action be not, as we see it is not, all in one direction, but in two reciprocal directions, is not the corollary of the doctrine of the conservation of force to the effect that in its general features the mundane system shall remain as it is?

As to the sun to which we in this planet owe everything, since there is no actual evidence that his action upon us now is less powerful than it ever was, would it not be well, instead of being so much concerned in producing fuel for him (with such indifferent success), rather to take into consideration Dr. Clarke Maxwell's formulæ in physical optics, which lead to the conception that the solar radiation is an electro-magnetic action? If so, then the sun would be always receiving as much as he was giving, and would not be wasting his energy save on the dissimilar bodies (the planets) which circle round him. With respect to the celestial spaces in general he may possibly be insulated in the æther by his own heat like a drop of water in the spheroidal state. Meantime the 22nd of December may bring us some discovery as to his structure. J. G. M.

* See "A Sketch of a Philosophy," part I. p. 2. (Williams and Norgate.)

NOTES

THE foundation stone of the new building for Owens College, Manchester, was laid on the site which has been purchased in Oxford Street on Friday last. The ceremony was performed by the Duke of Devonshire, K.G. The building has been designed by Mr. Waterhouse, the architect of the Manchester Assize Courts and of the new Town-hall. The style of the building is Gothic. It will have accommodation for 600 day students, and for a much larger number of evening students. A sum of 102,000*l.* has been placed at the disposal of the building committee, 67,000*l.* of which is at present available for the erection of the college. The cost of the building is 90,000*l.*, so that a sum of from 25,000*l.* to 30,000*l.* is still required for building purposes.

WE greatly regret to have received intelligence of the death at Geneva, on the 18th inst., of Dr. Augustus Waller, F.R.S. Dr. Waller held a high place among those physiologists who have enriched their science by original research. He is best known for his important contributions to the physiology of the nervous system, and especially for the introduction of a new method of investigation applicable to various important objects of neurological inquiry, which, not only as used by himself, but in the hands of others, has tended materially to advance the knowledge of that department. We owe also to Dr. Waller original and valuable observations on various other physiological questions. In acknowledgment of his scientific labours he twice received the Monthyon Prize of the French Academy of Sciences, first in 1852 for a research, in which he was assisted by Prof. Budge, of Bonn, and again in 1856 for experiments, exclusively his own, showing an important relation between the nutrition of nerve-fibres and their connection with nervous centres. For these and other researches in Experimental Physiology the Royal Society awarded him one of the royal medals for 1860. Dr. Waller began professional life as a general practitioner in Kensington, but his growing passion for original inquiry in science led him to devote himself to it entirely, and with the exception of a short time that he was Professor of Physiology in Queen's College, Birmingham, he resided abroad, and enjoyed the intimacy of the most celebrated Continental physiologists, who thoroughly appreciated his merits. Latterly he went to reside at Geneva, and commenced practice as a physician, still, however, continuing his physiological pursuits. He died quite suddenly in a fit of Angina pectoris, to which complaint he had been for some time subject.

WE are able to state that the *Quarterly Journal of Science* has passed into the hands of Mr. W. Crookes, F.R.S., who will from the present time be sole proprietor and editor.

THE American Science Association met at Troy, N.Y., on August the 17th. Professors Agassiz, Dana, and Henry were unavoidably absent. Papers on subjects of scientific interest were read by Professors Stimson of Boston, Winchell of Michigan, Orton of Vassar, Bradley of Jersey City, and many others.

IN addition to the professorial chairs already instituted at the University of Otago, New Zealand, the council of that University have now resolved to institute a chair of natural science, the salary of which will be 600*l.* per annum, besides class fees, &c., commencing to run from the day of embarkation. No religious test will be required of candidates.

THE programme of the lectures of the Birmingham and Midland Institute for the forthcoming session include a large proportion of a scientific character, as will be seen from the following list:—1870. October 3 and 10, "The Movements of Gases," by Professor Odling. October 17 and 24, "Extinct Animals intermediate between Reptiles and Birds," by Professor Huxley. October 31 and November 7, "Erasmus Darwin and Anna Seward, their Works and their Friends," by George Dawson. December 5, "The Lost Tribes of Tasmania," by James Bon-

wick. 1871. February 13, "Primitive Civilisation," by E. B. Tylor. March 13 and 20, "The Astronomy of Comets," by Professor Robert Grant. March 27, "War Ships and their Guns," by E. J. Reed. March 3 and 10, "Recent Researches in Physical Science," by W. F. Barrett.

AMONG the lectures to be delivered during next session before the Exeter Literary Society are "Recent Researches in Spectrum Analysis," by Rev. R. Kirwan; "Queer Flames, and what they have to say for themselves," by C. Meymott Tidy; "The Orbs of Heaven," by W. F. Quicke; and "Stonehenge compared with some other megalithic monuments," by Rev. R. Kirwan.

WE have received a prospectus of a new American Scientific Journal, to be called the *American Journal of Microscopy*. It will endeavour to create an elevated taste for microscopic science among the people. It will give full descriptions of the construction and ways of using the various kinds of microscopes and other optical instruments, the selection, gathering, and mounting of the most beautiful and interesting microscopic objects, microscopical diagnosis of diseases, reports of microscopical societies, and, in short, everything pertaining to microscopy. This journal will be ready for subscribers and agents about the 1st of October, and will be issued in neat quarto form of sixteen pages, of suitable size for preserving and binding, printed on fine white paper, in clear type, at one dollar per year. It will be published at Chicago.

WITH respect to the "whale of the bottle-nose species" said to have come ashore at Burntisland, Prof. Turner states that the animal is not one of the toothed whales, but a small whalebone whale. The plicated belly and the fatty fin on the back, placed it amongst the *Balænopteridæ* or finner whales. The white baleen, dashed here and there with pink, the form of the skull, and the elongated cruciform breast-bone, proved it to be the *Balænoptera rostrata* or piked whale—the smallest in the baleen whales inhabiting the North Sea. The specific distinction of this cetacean was first established by the late Dr. Robert Knox, from a specimen taken in 1834 in the stake nets at Queensferry, who named it the *Balæna minumus borealis*. Several specimens have since that time been stranded on the coasts of the Firth. Mr. Prentice has, with great liberality, presented the skeleton to the Anatomical Museum of the University of Edinburgh.

MR. W. G. SMITH'S "Clavis Agaricinorum," a paper originally presented to the Woolhope Naturalists' Field Club, and already printed in the *Journal of Botany*, will be very acceptable to mycologists in the separate and more complete form in which it is now published. Mr. Smith follows essentially the system of Fries and Berkeley, in dividing the enormous genus of *Agaricus* (numbering 450 British species alone) into five series, termed partly from the colour of the spores, *Leucospori*, *Hyporhodii*, *Dermini*, *Pratellæ*, and *Coprinarii*. Each of these five series he then divides into ten sub-genera, distinguished by characters of the hymenophorum, each of the five series of ten sub-genera closely corresponding to every other such series. Out of the fifty sub-genera which would thus be derived, only thirty-two are at present known, the remaining eighteen links in the chain remaining to be discovered. A number of very clear and carefully-executed diagram-plates which are appended will be of great service in rendering intelligible to beginners the principles of the arrangement of this exceedingly difficult genus.

PROF. O. C. MARSH has reprinted from the *American Journal of Science and Arts* several papers of considerable interest of American palæontology; Notice of some fossil Birds from the Cretaceous and Tertiary formations of the United States; Notice of some new Mosasauroid Reptiles from the Greensand of New Jersey; and a description of a new and gigantic fossil Serpent (*Dinophis grandis*) from the Tertiary of New Jersey.

WE are informed from American sources that the Western Union Telegraph Company in America propose to co-operate with the War Department, for the rapid transmission of telegraphic reports of "the operation and reports of storms for the benefit of commerce on our Northern lakes and sea-board." The practical value of the Meteorological information that can thus be rapidly sent across the great continent of America, from the Pacific to the Atlantic, cannot but be very great, and there is every hope that the War department will close with the offer.

SIR RODERICK MURCHISON announced to the Geographical Section of the British Association at Liverpool the receipt of the following telegram relating to the safety of the *Germania*—a little vessel which sailed some time ago on an expedition to the Arctic regions, chiefly at the expense of the German geographers and naturalists:—"Freeden to General Sabine. Koldeway—wintered in East Greenland, 70°; sledged to 77°. Arrived at Bremen last night. Extensive results—best health. Hamburgh, Sept. 12."

THE Consul-general of Chile at Bogota, the capital of Columbia, has written to his government to ask for particulars on the "Baldia" plant, reputed to be a great specific against liver complaint.

THE Economic Society of Guatemala is endeavouring to promote agricultural education there by means of books.

ONE of the most disastrous earthquakes we have lately recorded is that in the large city of Batang, on the Kin-sha river in Tibet. It began on the 11th April, and there were various shocks until the 9th May at 10 A.M., when it shook the whole city, causing a universal conflagration. Above 10,000 people were burnt to death. Several neighbouring villages shared the same fate. Many public buildings were destroyed, including those of the French millionaires. After the great shock others came on, and the people remained camped out for five days.

A SLIGHT earthquake is reported on the 26th July at 11 to 5 P.M., at Managua in Nicaragua, produced it is supposed by the volcano of Momstombe, from which subterranean rumblings were heard nearly every night. The people of Leon state that they have noticed flames and smoke proceeding from the crater. This seems to be the same earthquake that is reported from the neighbouring Republic of San Salvador on the 27th July. There it was strong, and lasted some seconds. On the 28th there was another, at 11.30 A.M., of a more violent character, but no serious damage was done.

ON the morning of the 1st August there was a slight shock of earthquake at Calcutta at about 5 minutes to 10.

THE Arabian Nights are in progress of realisation. Deposed sultars and princesses in disguise are not wanting; and the gold diggings furnish nuggets as large as any holder of a magic lamp can want. Aladdin's jewellery is now under realisation. The Diamond diggings in the Vaal River are the scene of successful venture, and one company in six months has got 22,000/ worth of diamonds. Women and babies are now taken to this scene of fortune, where bands of music, billiard tables, and other accessories of pleasure, have already appeared. Aladdin had not a billiard table when shut up in the cave of diamonds.

THE BRITISH ASSOCIATION

LIVERPOOL, *Wednesday*

THE number of tickets issued has now increased to 2,800, very considerably more than was expected either by the officers of the Association or by the Liverpudlians. The roll of eminent foreigners is also much larger. In addition to those mentioned in my first letter we have now the company of Prof. Tchebichef, of St. Petersburg, one of the most distinguished of living mathe-

maticians, Prof Plateau of Bruges, Dr. Anton Dohrn of Jena, Prof. Von Baumhauer of Leyden, and others. Some of these have added greatly to the interest of the sections in which they took part, and have been very warmly received. Dr. Dohrn in particular was greeted with much warmth, from the fact that three weeks ago he was summoned to join the German army and had not since been heard of by his English friends. The statement of a local paper that Prof. Hofmann of Berlin is here is incorrect. A letter was, however, read from him in the Chemical section, soliciting contributions in kind from our chemical manufacturers for the needs of the German hospital service. A large audience met to-day in the Geographical section to hear Lord Milton read his paper on "Railway Routes across America;" and the lecture-room in which the Ethnological sub-section holds its meetings was crammed whilst Sir John Lubbock was discoursing on "Stone Implements from Western Africa." The small and inconvenient Crown Court in St. George's Hall devoted to Section A had a much larger complement than usual while the Rev. F. Howlett read his paper on "Solar Spots," and exhibited his elaborate diagrams, showing the great interest now taken by the public in solar phenomena; although, at the same time, Section G offered a counter attraction, especially to the members connected with the town, in Mr. Mackie's and Sir E. Belcher's papers on "The Unprotected State of Liverpool." The heavy rain and thick fog of yesterday morning have both cleared off, and the weather is again everything that could be desired.

The subject of Spontaneous Generation is undoubtedly *the* question of the meeting of the British Association for 1870. The title of the paper by Professor Huxley which headed yesterday's list in the department of Zoology and Botany, did not appear to bear directly upon it, and yet it was generally understood that it would reopen the subject. The President's discourse, for he had scarcely a note before him, was a popular account of the mode of development and form of those minute structures which the microscope reveals in such prodigious numbers in infusions containing organic matter, *Penicillium*, *Torula*, *Bacterium*, and *Vibrio*. He adduced arguments in favour of the theory that these various bodies are not distinct organisms, but are different modes of development of the same substance, and a more admirable and luminous exposition, it was generally admitted, has seldom been delivered. In the course of his remarks, Prof. Huxley took occasion to explain the difference between the "Brownian" motion of the molecules of inorganic matter, and the vital motions of living matter, and expressed his conviction that the motions observed by Dr. Bastian in the infusions which had been subjected to long-continued high temperatures, were referable to the former and not to the latter cause. During the discussion which followed, Dr. Bastian entered the room, but when called on by the president of the section, preferred deferring his reply till the following day. This morning Dr. Bastian gave an account of his experiments on the contents of hermetically sealed cases of preserved meats, with which the readers of NATURE are already familiar, and reiterated his conclusion that the facts he had elicited were such as to throw on the Biogenists the burden of proof that life did not really, as was apparently the case, originate *de novo* from lifeless materials. Professor Huxley was not able to be present at this discussion, but a somewhat sharp passage of arms took place between Dr. Bastian and Professor Tyndall, each maintaining his well-known view respecting the atmospheric germ theory. The reply of Prof. Tyndall, "Prof. Huxley's lieutenant," as he was described by the president of the section, was not generally accepted as conclusive, in consequence of his apparently not having made himself thoroughly acquainted with the facts of the series of experiments performed by Dr. Bastian.

Several of the sections had got through their programme of papers yesterday, but the majority sat this morning, and Section D even intruded into the time fixed for the meeting of the General Committee which concluded the business of the meeting. In opening the business of the committee, Professor Huxley remarked that whatever reports are issued in the name of the committee of recommendations, it should be clearly understood that they are issued without any sanction of the Association, and that the responsibility of these reports, and the conclusions that may be drawn from them, rest entirely with the authors of the reports. The Association does not for one moment endorse views on subjects on which persons of eminence may hold different opinions. All that the Association is responsible for, is to place in a position for making reports gentlemen who are competent to make them; what these gentlemen say is entirely on their own responsibility. Grants were then ordered for various purposes, as recommended by the committee of recommendation, in accordance with the list which will be found in another column. A number of resolutions not involving grants of money, which came up from the various sections, were also passed.* The most important among these were:—A resolution on the subject of Vivisection, and appointment of a committee, from Section D; a resolution, brought up from Sections A and B, requesting the Council to co operate with the Councils of the Royal and Astronomical Societies urging on the Government the propriety of reconsidering their refusal to aid in the observation of the approaching Total Eclipse of the sun by the grant of a vessel; a resolution from Sections A, B, and G, with reference to the proposed foundation by the Government of an Engineering College, to the effect that it is undesirable that any fresh grants should be made for purposes of scientific instruction until the Royal Science Commission has reported. The only resolution which excited much discussion was one from Section F in favour of the adoption of a compulsory metric system of weights and measures. An amendment was moved by Mr. Hawksley, and seconded by Professor Rankine, that the recommendation should be limited to the adoption of the metric system for international purposes, on the ground that a binary system is now, and always will be, the mode adopted by uneducated persons in all countries for their own purposes, as being the simplest. The amendment was adopted by a considerable majority.

Saturday

Those who were present at the meeting of the British Association which has just concluded its sittings, speak of it as presenting some points of favourable contrast with preceding meetings. There were still a good many papers presented of a purely technical character, which would have been far more suitable for the transactions of one of the learned societies, and which were perfectly unsuited for the miscellaneous audience collected to hear them. On the other hand there were not a few, perhaps a larger number than on previous occasions, which treated purely scientific subjects in a philosophical manner, calculated not only to interest but to enlarge the minds of all who had the pleasure of listening to them. I may illustrate my remark by reference to one division only, Section D. A glance over the titles of the papers read in this section will at once suggest several belonging to the former category. On the other hand, Dr. Brown-Sequard's paper on "Various Alterations of Nutrition due to Nervous Influence," and Prof. Flower's on the "Connection of the Hyoid Arch with the Skull," though treating of subjects belonging to pure science, were discussed in a manner which carried with them a non-scientific audience, who cannot have failed to carry away some ideas altogether new to them. In the same manner Prof. Huxley's

* Owing to these resolutions not having been yet sanctioned by the Council, we are compelled to defer their publication for the present.

"History of the Development of the Lowest Forms of Infusorial Life" was a model of clearness and succinctness; and the President himself paid a compliment to Mr. Bennett's paper, which followed, on the "Theory of Natural Selection looked at from a Mathematical Point of View," as the first attack on the hypothesis conceived in a philosophical spirit, and such a paper as it is the special object of the Association to bring out. The great question of the meeting, that of "Spontaneous Generation," has already occupied sufficient space in our columns; we look to the Liverpool meeting as the starting-point from which the discussion must in future be carried on in a truly philosophical and inductive spirit, free from the dogmatism which has hitherto surrounded it.

The attitude of Liverpool towards the Association has been somewhat of a puzzle to its members. If we were to judge from the remarks heard in the streets and from the ordinary visitors at the hotels, and the "intelligent policemen" who were posted here and there to direct the wandering visitor, we might suppose that a meeting of the British Association was a monthly occurrence in Liverpool, so utterly indifferent did they appear to it. The prevalent opinion appeared to be that we were another Church Congress in some sort of disguise. That there is a public in Liverpool who watched its proceedings with intelligence and interest was evident; but this, at least, is certain, that such a public has no representative in the Liverpool press. Let any one who wishes to see what the papers say about the Association take up the *Liverpool Courier* of this morning, where they will find a leader devoted to a ponderously jocose reply to an innocent remark of ours last week. "Save us from our friends" was the remark with which we laid down the following comment on the Association:—"The philosophers have come and gone, and Liverpool is at peace. They had a week of tremendous talk—tremendous not only as regards volubility, which is a blight we are well accustomed to in Liverpool, but also as regards the technical ponderosity of the themes. However, it is over at last, and though we entertain the most devout admiration for science and scientific people, a sigh of relief escapes involuntarily as we speed the parting guest. It was impossible that we could live long at the high pressure of the past week. It was more than human nature—of course we except scientific human nature, which is on a higher round in the Darwinian ladder than we poor cotton-dealers and traffickers—could bear for another seven days. The philosophy was too exalted for our earthy intellects, and we are bound to confess that the local savans—we have quite a battalion of the genus, such as they are—manifested signs of weariness under the tempest-torrent of imported intelligence. Of course it was right that the personages who hang on to the skirts of philosophy should be members of the British Association; but what a change between the first and the last day! Mathematics and biology were a gay pastime in the initial stage, the wise men and the learned ladies were positively vivacious over the germ-theory, and not a few were prepared to enter the lists against any opponent of abiogenesis. But this enthusiasm could not be sustained, for there was no basis of real intelligent interest, and the animation waned as the stream of science still flowed on."

The reporter of the *Courier* clearly found that he was getting beyond his depth, and wisely absented himself from the sittings during the last few days, or he would have known that the interest showed no signs of abatement. I venture to predict that those Liverpudlians for whom the meeting was not altogether "caviare" have been so well pleased with the success of the meeting that in due time we shall have another invitation to pay the town a visit, when I hope large numbers of those who have now dispersed will again meet.

It will be seen that the annual grant of 600*l.* for the purposes of the Kew Observatory has only two more years to run. We must trust that the additional means thus placed at the disposal of the Council will be applied to purposes directly connected with the real advancement of science, and will not be frittered away in bricks and mortar.

After holding the concluding general meeting which followed that of the general committee, the visitors to the Association rapidly dispersed; a small proportion, however, staying to avail themselves of one or other of the excursions which were arranged for the Thursday. In this respect next year's meeting at Edinburgh will afford a much larger scope for the lovers of the picturesque. B.

Resolutions of the General Committee

Applications for Reports and Researches not involving Grants of Money

That Prof. R. B. Clifton, Mr. Glaisher, Mr. Huggins, Dr. Matthiessen, Prof. W. Hallows Miller, Dr. Balfour Stewart, Mr. G. Johnstone Stoney, Lieut.-Col. Strange, and Sir J. Whitworth, Bart., be a Committee for the purpose of reporting on Metric Standards, in reference to the communication from Prof. Jacobi, appended hereto:—

"The Academy of Sciences of St. Petersburg, observing that the Standard Metric Weights and Measures of the various countries of Europe and of the United States, differ by sensible, though small, quantities from one another, express the opinion that the continuance of these errors would be highly prejudicial to science. They believe that the injurious effects could not be guarded against by private labours, however meritorious, and they have therefore recommended that an international commission be appointed by the countries interested to deal with this matter. They have decided to bring the subject before the Russian Government, and have appointed a Committee of their own body, who have drawn up a careful Report containing valuable suggestions; and they have deputed Prof. Jacobi to lay this Report before the British Association, and to request the Association to take action in reference to it."

That Dr. Anton Dohrn, Prof. Rolleston, and Mr. P. L. Sclater be a committee for the purpose of promoting the foundation of zoological stations in different parts of the world, recognising the foundation of a zoological station at Naples as a decided step in this direction; that Dr. Anton Dohrn be the Secretary.

That the committee of Section D be requested to draw up a statement of their views upon Physiological Experiments in their various bearings, and that this document be circulated among the members of the Association.

That the said committee be further requested to consider from time to time whether any steps can be taken by them or by the Association, which will tend to reduce to its minimum the suffering entailed by legitimate physiological inquiries, or any which will have the effect of employing the influence of this Association in the discouragement of experiments which are not clearly legitimate on live animals.

Resolution passed by the Committee of Section D (Biology):

"That the following gentlemen be appointed a Committee for the purpose of carrying out the suggestion on the question of Physiological Experiments made by the General Committee:—Professor Rolleston, Professor Lawson, Professor Balfour, Dr. Gamgee, Professor M. Foster, Professor Humphry, Professor W. H. Flower, Professor Sanderson, Professor Macalister, and Professor Redfern; that Professor Rolleston be the Secretary, and that they be requested to report to the Committee."

Involving Application to Government

That Sir R. I. Murchison, Bart., Sir Charles Lyell, Bart., Mr. Findlay, and Admiral Sir John D. Hay be a committee for the purpose of bringing to the notice of the Commissioners of the Admiralty the importance of revising the survey of the west coast of South America, with a view to ascertaining what changes have taken place in the levels since the recent great earthquakes on that coast; that Mr. Clements Markham be the secretary.

That Prof. Jevons, Mr. R. Dudley Baxter, Sir John Bowring, Mr. J. T. Danson, Mr. James Heywood, Dr. W. B. Hodgson, and Prof. Waley be a committee for the purpose of urging upon her Majesty's Government the expediency of arranging and tabulating the results of the approaching census in the three several parts of the United Kingdom in such a manner as to admit of ready and effective comparison; that Mr. Edmund Macrory be the secretary.

SYNOPSIS OF GRANTS OF MONEY appropriated to Scientific Purposes by the General Committee at the Liverpool Meeting in September 1870. The names of the members who would be entitled to call on the General Treasurer for the respective Grants are prefixed:

	£	s.	d.
Kew Observatory			
The Council.—Maintaining the Establishment of Kew Observatory	600	0	0
Mathematics and Physics			
*Brooke, Mr.—British Rainfall	50	0	0
*Thomson, Professor Sir W.—Underground Temperature	150	0	0
*Tait, Professor.—Thermal Conductivity of Iron and other Metals	20	0	0
*Thomson, Professor Sir W.—Tidal Observations	100	0	0
*Glaisher, Mr.—Luminous Meteors	30	0	0
Crossley, Mr.—Observations of Lunar Objects	20	0	0
Herschel, Sir J.—Recomputation of the Gaussian Constants for 1839	50	0	0
Stewart, Professor B.—Standard Measures of Electrical Capacity	20	0	0
Hockin, Mr.—Standard Electrodynamometer	20	0	0
Thomson, Professor Sir W.—Standard Potential Gauge	20	0	0
Chemistry			
Williamson, Professor.—Reports of the Progress of Chemistry	100	0	0
Brown, Professor Crum.—Thermal Equivalents of the Oxides of Chlorine	25	0	0
Geology			
*Lyell, Sir C., Bart.—Kent's-Cavern Exploration	150	0	0
*Duncan, Dr. P. M.—British Fossil Corals	25	0	0
*Symonds, Rev. W. S.—Sedimentary Deposits in the River Onny	10	0	0
*Mitchell, Mr. W. S.—Leaf-beds of the Lower Bagshot series	20	0	0
Thomson, Mr. James.—Sections of Fossil Corals	20	0	0
Scott, Mr. R. H.—Mesozoic Deposits of Omenak, North Greenland	50	0	0
Woodward, Mr. H.—British Fossil Crustacea	25	0	0
Busk, Mr.—Fossil Elephants of Malta	25	0	0
Biology			
*Carruthers, Mr.—Fossil Flora of Britain	25	0	0
*Sharpey, Dr.—Physiological Action of Methyl Compounds	25	0	0
*Sclater, Mr.—Record of the Progress of Zoology	100	0	0
*Foster, Professor M.—Heat Generated in the Arterialisation of Blood	15	0	0
Balfour, Professor.—Effect of the Denudation of Timber on the Rainfall in North Britain	20	0	0
Geography			
Murchison, Sir R. I., Bart.—Exploration of the Country of Moab	100	0	0
Statistics and Economic Science			
*Bowring, Sir J.—Metrical Committee	25	0	0
	£1840	0	0

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

Abstract of an Investigation of the Mathematical Theory of Combined Streams.—Professor W. J. M. Rankine, F.R.S. The object of the investigation of which this is an abstract is to extend to combinations of any number of streams of fluid, whether liquid, vaporous, or gaseous, the principles which have been applied to combinations of two streams by previous authors, and especially by Professor Zeuner, in his treatise entitled "das Locomotiven-Blasrohr" (Zürich, 1863). Several component streams of fluid, each coming through its own supply-tube and nozzle, are led in directions parallel to each other, into one end of a cylindrical space called the junction-chamber, in which they mingle so as to form a resultant stream; and that resultant stream escapes from the other end of the junction chamber through an orifice called the throat. The dynamical principle upon which the motion depends is that of the equality of impulse and momentum. The aggregate momentum per second of the component streams is found by multiplying the mass of fluid which comes from each nozzle in a second by its velocity, and adding together the products. The momentum of the resultant stream is the product of the mass of fluid discharged from the throat in a second, into the velocity at the throat. The difference of these two momenta is equal to the impulse per second exerted in the junction-chamber, which impulse is found by multiplying the area of the throat by the difference between the intensities of the pressure at the nozzle end and at the throat end of the chamber respectively. If there be a gain of momentum, the pressure at the throat is less than at the nozzles; if there be a loss of momentum, the pressure at the throat is greater than at the nozzles.

There is always a loss of energy, which is expended in producing eddies; unless the velocities of the component and resultant streams are all equal to each other. The amount of that loss can be calculated in any given case, by the help of the principle already stated; and that principle being expressed in the form of an equation, and taken together with another equation expressing the equality of the mass discharged at the throat to the sum of the masses which come through the nozzles, affords the means of solving various problems as to combined streams.

Abstract of a Paper on the Thermo-dynamic Acceleration and Retardation of Streams.—Professor W. J. M. Rankine, F.R.S.* The object of this paper is to state in a more general and comprehensive form than has hitherto been done to my knowledge, a thermo-dynamic and hydro-dynamic principle of which many particular cases are well known and understood. That principle may be stated as follows:—

In a steady stream of any fluid, the abstraction of heat at and near places of minimum pressure, and the addition of heat at and near places of maximum pressure, tend to produce acceleration; the addition of heat at and near places of minimum pressure, and the abstraction of heat at and near places of maximum pressure, tend to produce retardation; in a circulating stream, the quantity of energy of flow gained or lost in each complete circuit is equal to the quantity of energy lost or gained in the form of heat; and in the absence of friction, the ratios borne by that quantity to the heat added and the heat abstracted (of which it is the difference) are regulated by the absolute temperatures at which heat is added and abstracted, agreeably to the second law of thermo-dynamics.

Amongst particular cases of the thermo-dynamic acceleration and retardation of streams, the following may be specified:—

Acceleration by the addition of heat at and near a place of maximum pressure; the draught of a furnace; and the production of disturbances in the atmosphere in regions where the ground is hotter than the air.

Retardation by the abstraction of heat at and near a place of maximum pressure; the dying away of atmospheric disturbances in regions where the ground is colder than the air.

Acceleration by the abstraction of heat at and near a place of minimum pressure; the injector for feeding boilers, in which a jet of stream, being liquefied by the abstraction of heat, is enabled not only to force its way back into the boiler, but to sweep a current of additional water along with it; also, to a certain extent, the ejector-condenser.

The conduction of heat from the parts of a stream where the pressure and temperature are highest, to the parts of the same stream where the pressure and temperature are lowest, produces, according to the foregoing principles, a gradual and permanent retardation of the stream, independently of the agency of friction; and this is accompanied by the production of heat to an amount equivalent to the lost energy of flow.

SECTION B.—CHEMICAL SCIENCE

On the Weldon Process for the Manufacture of Chlorine.—Mr. W. Weldon, F.C.S., the author, said the process was one for the manufacture of chlorine by means of a perpetually-regenerated reagent consisting mainly of a compound containing the elements of peroxide of manganese and lime, and which was previously unknown. He had described the process last year at

* Re-appointed.

* Printed in full in the "Philosophical Magazine" for October, 1870.

the Exeter meeting, when it was in operation at only two works. It is now either in operation or on the point of being adopted at almost all the works in this country, and at a number of works in France and Germany. In consideration of the fact that the production of chlorine will probably be completely revolutionised by the Weldon process, and considering, likewise, that chlorine is largely prepared in the neighbourhood of Liverpool and in other parts of Lancashire, the author had agreed to the request of Prof. Roscoe that he should submit to the section a brief account of the practical results, which the process had been found to yield under more extended experience, and of the development which it had undergone during the year. The author first described the apparatus employed, and exhibited a small model of it, and then proceeded to state that the chloride of manganese, which results in the ordinary preparation of chlorine, and which is generally acid, is neutralised by adding to the liquor finely-divided carbonate of lime. The liquor then consists of a neutral mixed solution of chloride of manganese and chloride of calcium, and contains, in suspension, a large quantity of sulphate of lime and smaller quantities of oxide of iron and alumina. The clear solution, after settling, is oxidised by passing into it a blast of atmospheric air from a blowing engine, and heated, if necessary, by a current of steam. Milk of lime is then run into the oxidiser until the liquid ceases to give a manganese reaction with solution of bleaching powder. A further quantity of milk of lime is added, and ultimately from eighty to eighty-five per cent. of the manganese is converted into peroxide. The mixture is allowed to settle, the chloride of calcium solution forming the supernatant liquid is run off, and the residual black mud containing the manganese peroxide is used in the stills where hydrochloric acid is decomposed and chlorine gas produced. A residual liquor such as was commenced with, results, and the round of operations is begun again; and so on, time after time indefinitely. After giving an outline of his mode of treating still liquor, Mr. Weldon described at considerable length the details of the process, both as to quantities of materials employed and obtained, and the nature of the chemical compounds formed at different stages of the process. As explained by Prof. Roscoe, the principle upon which the process depends is that, although when alone, the lower oxides of manganese cannot be oxidised by air and steam under the ordinary pressure to the state of dioxide, yet this is possible when one molecule of lime is present to each molecule of oxide of manganese. The manganous oxide is precipitated from the still liquors with the above excess of lime, and by the action of steam and air on this, a black powder, consisting of manganese dioxide and lime, or calcium manganite (MnO_2 Ca O), is formed. This compound is again capable of generating chlorine from hydrochloric acid, and thus the chlorine process is made continuous with a working loss of only 2½ per cent. of manganese.

A short discussion followed the reading of the paper, in the course of which Mr. Gossage stated that his experiments on the improvement of the chlorine process had extended over thirty-five years, and he was glad Mr. Weldon's efforts had been attended with such a large measure of success.

Air Pollution from Chemical Works. — Mr. Alfred E. Fletcher, F.C.S., one of the inspectors under the Alkali Act. The author remarked that during the progress of many manufacturing processes gases or vapours were given off, which passing into the surrounding air, polluted it and rendered it more or less unfit for animal or vegetable life. If all noxious vapours were to be suppressed by the summary stoppage of the manufacturing processes causing them, we must dispense with the use of a variety of useful substances; indeed, it might be said that every manufacture was accompanied at its birth by some offensive smell or smoke. Still, the public were right in requiring that noxious vapours should be reduced to a minimum. Those who were not acquainted with manufactures would be surprised at the large amount of noxious vapours discharged from works. He then referred to the direction which he thought future legislation should take, his opinion being, he said, guided by the observations of Dr. Angus Smith, in his reports as chief inspector under the Alkali Act. In places where complaints were made against manufacturers by farmers, as to damage to their crops by corrosive smoke, let the district be called "a manufacturing district," upon the requisition of a certain number of inhabitants. To such district an inspector should then be appointed, who should have power at any time to ascertain the nature and amount of gases escaping from the various works. At the end of each month, or a longer period, the inspector should publish a list of all the works in his district, with a number indicating the average amount of acid vapour he had found upon his visits. There the inspector's duties should terminate, as he should be neither prosecutor nor judge, but merely publish the facts ascertained, which the farmer himself could never have gathered. He contended that such a plan would be beneficial in its operation. It would be universal in its application, and would embrace every description of manufacturing works. Hitherto legislation had been partial. There was an Alkali Act, but it regulated the alkali manufacture only.

In the discussion it was strongly urged that over-legislation on this question should be avoided, and that the crops do not suffer to the extent which was sometimes imagined. The Alkali Act had been so far beneficial as to call the attention of manufacturers to the subject of air pollution, and they had been induced to employ improved apparatus at their works.

On the Phenomena of the Crystallisation of a Double Salt.—Mr. J. Berger Spence. Mr. Spence said that hitherto many scientific chemists had doubted the possibility of producing soda alum; but the results of upwards of fifty experiments which he had made conclusively showed that this salt can be produced under certain circumstances. The principal point of interest in Mr. Spence's paper consisted in the discovery, made by him, that the crystals are produced from an amorphous mass, which is formed when the solution is prepared at high densities. The immediate result of this discovery may be that the large quantity of ammonia which is now used in the production of alum will be displaced in favour of soda, and that the valuable fertiliser, ammonia, which has no intrinsic value in alum, will be given to the soil, which, in an economic point of view, will be of considerable advantage to the country.

Section C.—Geology

Report on British Fossil Corals.—Prof. P. M. Duncan. The distinction between the palæozoic and later coral faunas was shown to be not so exact as was supposed, and that the aporose and perforate corals existed in the palæozoic rocks, as well as rugose and tabulate forms, which latter had closely allied recent analogues. The report contained a new classification of the Tabulata, and entered into the Alcyonarian characters of the *Chætetinæ* and the Hydrozoan characteristics of the *Milleporidæ*.

On the Fossil Elephants from Malta.—Dr. Leith Adams. After referring to his former reports, communicated to the Association in 1865 and 1867, in which the situation and nature of the Maltese ossiferous caves were described, Dr. Leith Adams now submitted further observations on the elephantine remains which has been collected by him in enormous quantities in those localities, and pointed out the important results that might be expected to flow from the comparison of these materials with those which had been brought by Captain Spratt from Zebbug, in which the late Dr. Falconer, in the year 1862, had discerned the existence of a dwarf or pigmy species, together with that of a larger form. Subsequently, Mr. Busk, on proceeding to work out Captain Spratt's collection in detail, found reason to discriminate three distinct forms, one of the average dimensions of the existing African and Asiatic species, and two others, differing from each other not only in size, but, as it would appear, in other osteological characters, but both of comparatively small or dwarf stature. The Zebbug collection, however, afforded but very scanty evidence with respect to the largest form, whilst that made by Dr. Leith Adams abounds with its remains, and will consequently allow of the correct determination of the true relations of that form with the two smaller forms found in association with it, as well as with other existing and extinct species, and especially with *E. antiquus*, whose relations up to the present time have remained obscure. Mr. Busk, by whom the paper was communicated, exhibited specimens selected from Dr. Adams' collection, proving the existence of the three forms above adverted to.

Report on the Exploration of Kent's Cavern.—Mr. W. Pengelly. During the past year the committee had investigated the only portions of the eastern division of the cave which had remained unexplored. These portions had been called by the Rev. J. MacEnery, the North and South Sally Ports in the belief that they led to external openings. The South Sally Port has a southeast direction into the hill, and away from the hill-side; it occupies a space of 80 feet by 40 feet. It was filled with, first, a red cave-earth from 12 to 20 inches thick; second, a stalagmitic floor from 1 to 24 inches thick; and third, a cave-earth of unknown depth, but exceeding 5 feet. The diggings yielded a

large number of bones—including several birds and a few fish—portions of antlers, and about 1,400 fragmentary and perfect teeth, some of them still attached to the jaw-bones. The teeth belonged to the following animals:—Horse, hyena, rhinoceros, bear, sheep, badger, fox, rabbit, elephant, deer, lion, ox, hare, and pig. Agglutinated lumps of wings and elitra of beetles. Besides these, twenty-one flint implements and flakes were found. The North Sally Port covers an area of 86 feet by 84 feet, and passes out to an opening in the eastern slope of the hill. The three layers described in the South Sally Port are found in this opening also, and the remains obtained in the excavations were the same as and in like proportions to those found in the South Sally Port. Smerdon's Passage was determined to be the entrance to the North Sally Port, and also to a previously unsuspected passage. Numerous remains were found in this passage.

Section D.—Biological Science

Professor Rolleston's Inaugural Address. (Continued from p. 427.)

Pathology has made a return to Physiology for much service she has received, and this in the following directions. Dr. W. Ogle has thrown much light on the physiology of the cervical sympathetic nervous system by his record of a pathological history to be found in the recently issued volume (vol. liii.) of the "Medico-Chirurgical Transactions." The rough and cruel experimentation of war has had its vivisections utilised for the elucidation of the physiology of nerves, and especially of their trophic function, by the valuable volume issued by the American Sanitary Commission, under the editorship of Dr. Austin Flint. Dr. Broadbent has done something towards elucidating the question of the localisation of functions in particular parts of the cerebral convolutions which was so extensively and so very exhaustively discussed at Norwich by his paper in our most useful and comprehensive Cambridge "Journal of Anatomy and Physiology," May 1870, "On the Cerebral Convolutions of a Deaf and Dumb Woman."

I take this opportunity of mentioning two valuable papers on the very practical question of the influence of the vagus upon the heart's action. One of these is a German paper by a gentleman who is a zoologist and comparative anatomist as well as a physiologist, Dr. A. B. Meyer, "Das Hemmungsnervenssystem des Herzen" is the title of his memoir, a separate publication as I think; the other is an abstract of a paper [I have not seen the paper published *in extenso* as yet] by Dr. Rutherford, "On the Influence of the Vagus upon the Vascular System," published in the Cambridge journal just referred to. Especially do I think Dr. Rutherford's view as to the vagus acting centrifugally as regards the stomach, and carrying stimulus, not thither but thence, to the medulla oblongata, which stimulus is then radiated downwards by a route formed distally by the splanchnic nerve, so as to produce inhibitory vascular dilatation in the neighbourhood of the pepetic cells, as worthy of attention.* A considerable number of the papers which will be read before this Section, indeed a considerable part of the Section itself, will be devoted to the Natural History of Man. Nothing, I apprehend, is more distinctive of the present phase of that "proper study of mankind" than the now accomplished formation of a close alliance between the students of archæology strict and proper and the biologist with the express purpose of jointly occupying and cultivating that vast territory. Literature and art and the products of the arts furnish each their data to the ethnologist and anthropologist in addition to those which it is the business of the anatomist, the physiologist, the palæontologist, and the physical geographer, to be acquainted with; nor can any conclusion attained to by filling up any single one of those lines of investigation be considered as definitely absolved from the condition of the provisional until it has been shown that it can never be put into opposition with any conclusion legitimately arrived at along any other of the routes specified. In political alliances the shortcomings of one party necessarily hamper and check the advance of the other; a failure in the means or in the perseverance of one party may bring the joint enterprise to a premature close; mutual forbearance, not to dwell longer upon extreme cases, may finally be as effectual in slackening progress as even mutual jealousies. No such disadvantages attach themselves to the alliance of literature with science, as the German "Archiv für Anthropologie," issued to the world under the joint management of Ecker the biologist and Lindenschmidt the antiquarian, will show any one who consults its pages, replete with many-sided but not superficial, multifarious but never inaccurate, information.

The antiquary is a little prone, if he will allow me to say so, when left alone, to make himself but a connoisseur; the historian, whilst striving to avoid the Scylla of judicial dulness, slides into the Charybdis of political partizanship; and the biologist not rarely shows himself a little cold to matters of moral and social interest, whilst absorbed in the enthusiasm of speciality. The combination of minds varying in bent is found efficacious in correcting these aberrations, and by this combination we obtain that white and dry light which is so comforting to the eye of the truth-loving student, to say nothing as to its being so much stronger than the coloured rays which the work of one isolated student has sometimes cast upon it from the work of another. It would be invidious to speculate, and I have forborne from suggesting whether the literary contingent in the conquering though composite army has learnt more from observation of the methods and evolutions of the scientific contingent, or the scientific man from the observation of the literary; it is, however, neither invidious nor superfluous to congratulate the general public upon the necessity which these, like other allies, have been reduced to, of adopting one common code of signals, and discarding the exclusive use of their several and distinctive technicalities. Subjects of an universal interest have thus come to be treated, and that by persons now amongst us, in a language universally understanded of the people. I have been careful to include the Palæontologist amongst the scientific specialists whose peculiar researches have cast a helpful and indeed an indispensable light upon the history of the fates and fortunes of our species. But it is not organic science only which anthropology impresses into its service, and it would be the sheerest ingratitude to forget the help which the Mineralogist gives us in assigning the source whence the jade celt has come or could come, or to omit an acknowledgment of the toil of the analytical chemist, who has given the percentage of the tin in the bronze celt, or in the so-called "leaden" and therefore Roman coffin.

I am very well aware that many persons who have honoured me by listening to the last few sentences, have been thinking that it is at least premature to attempt to harmonise the two classes of evidence in question; and that the best advice that can be given to the two sets of workers severally is, that they should work independently of each other. Craniography is said, and by irrefragable authority, to be a most deceptive guide; works and articles on ethnology tell us stories of skulls being labelled, even in museums of the first order of merit, with such Janus-like tickets as "Etruscan Tyrol *or* Inca Peruvian;" and one of the most celebrated anthropotomists of the day, has been so impressed with the fact that Peruvian as well as Javanese and Ethiopian skulls may be found on living shoulders within the precincts of a single German university town, that he has busied himself with forming a pseudo-typical ethnological series from the source and area just indicated. Great has been the scandal thence accruing to craniography, and the collector of skulls has thence come to be looked upon as a dilettante with singular ghoul-like propensities, which are pardonable only because they relate only to savage races of modern days, or to cemeteries several hundred years old, but which are not to be regarded as being seriously scientific. Now to me the existence of such a way of estimating such a work appears to argue a sad amount of ignorance of the laws of the logic of practical life, or, indeed, of the chapters on "approximate generalisations," which any man, however unpractical, can read in a treatise on logic. A man's features and physiognomy are instinctively and intuitively, or, if you prefer so to put it, as a result of the accumulated social experiences of generations of men, taken as a more or less valuable and trustworthy indication of his character; were this not so, photographers would not, as I apprehend, and hope they do; make fortunes, yet the face is at least as often fallacious as an index of the mind as the skull is fallacious as an index of race. The story of the misconception by a physiognomist of the character of Socrates is familiar to us, as I think, from Lemprière's Dictionary; and it may serve to parallel the story which Blumenbach and Tilesius tell us of the exact correspondence of the proportions of a skull from Nukahiva with those of the Apollo Belvidere. The living faces in a gaol again, to put the same argument upon other grounds, are as dangerous to judge from as are the skulls in the museum; yet every detective is something like a professor of physiognomy, and most of them could

* Since writing as above I have seen, but have not read, a paper by Dr. Coats in Ludwig's "Arbeiten aus der Physiologischen Anstalt zu Leipzig" for the present year. The Wurtzburg Physiological Laboratory Reports for 1867-1868, contain, as is well known, a series of papers on this subject.

write a good commentary on Lavater. The true state of the case may, perhaps, be represented thus:—A person who has had a large series of crania through his hands, of the authenticity of which, as to place and data, he has himself had evidence, might express himself, perhaps, somewhat to the following effect if he were asked whether he had gathered from his examination of such a series any confidence as to his power of referring to, or excluding from, any such series, any skull which he had not seen before. He might say, "the human, like other highly-organised types of life, admits of great variety, aberrant forms arise, even in our own species, under conditions of the greatest uniformity possible to humanity; amongst savages great variety exists (see Bates, 'Naturalist on the Amazons,' ii. p. 129), even though they all of them may live the same 'dull grey life,' and die the same 'apathetic end;' and consequently it may never, except in the case of Australian or Esquimaux, and perhaps a few other crania, be quite safe to pledge oneself as to the nationality of a single skull. Still there is such a thing as craniographical type, and if half-a-dozen sets, consisting of ten crania apiece, each assortment having been taken from the cemeteries of some well-marked nationality, were set before me, I would venture to say, after consultation and comparison, especially with such assistance as that which an assembly such as this might furnish me with, that it might be possible to show that unassisted cranioscopy, if not invariably right, even under such favourable circumstances, was nevertheless not wrong in a very large number of cases." If it is true on the one hand that *in generalibus latet error*, it is true on the other that security is given us by the examination of large numbers for the accuracy and reliability of our averages, a principle which, Gratiolet informs us, is thoroughly recognised in Chinese metaphysics, and which he has formulated in the following words:—"L'invariabilité dans le milieu s'applique à tout. La verité n'est point dans un seul fait mais dans tous les faits; elle est dans les moyennes, c'est-à-dire dans une suite d'obstructions formalées après le plus grand nombre d'observations possibles." (Mémoire sur les Plis cérébraux, p. 93). The natural history sciences do not usually admit of the strictness which says that an exception, so far from proving a rule, proves it to be a bad one; rather are we wise in saying that in them at least the universality of assertion is in an inverse ratio to that of knowledge, and that the sweeping statements dear, as Aristotle long ago remarked (Rhetoric, ii. 21. 9 & 10.; ii. 22. 1.), to a class which he contrasts with the educated, are abhorrent to the mind of organic nature. It is true enough, as is sometimes said, that when opinions and assertions are always hooped in by qualifications, the style becomes embarrassed, and the meaning occasionally hard to be understood; but this difficulty is one which lies in the very nature of the case, and the real excellence of style does not consist in its lulling the attention and relieving the memory by throwing an alliterative ring on to the ear, but in the furnishing a closely fitting dress to thought, and an accurate representation of actual fact.

If we are told that the attempt to harmonise the results, not merely of cranioscopy, but of any and all natural science investigation, with the results of literary and linguistic research, is needless and even futile, this is simply equivalent to saying that one or other of these methods is worthless. For as Truth is one, if two routes, purporting both alike to lead to it, do not sooner or later converge and harmonise, this can only be because one or other of them fails to impinge upon the goal. It is true that by certain lines of investigation light is thrown upon a problem, but at a single point, and that all further prosecution of investigation along that line will but lead us off at a tangent. Still the throwing of even a single ray upon a dark surface is an achievement with a value of its own; and it is a cardinal rule in our sciences never to ignore the existence of seemingly contradictory data, in whatsoever quarter they may show themselves. For what would be said of an investigator of a subject such as physical geography, who should declare that he would pay no attention but to a single set of data, when he was discussing whether a particular archipelago had been formed by upheaval, or should be held to be the fragments and remnants of a disrupted continent; and that if geological evidence was in crying discord with his interpretation of the facts of the distribution of species, it was not his business to reconcile them. He would be held to have neglected his business, as you may see by a reference to Mr. Bentham's Address to the Linnean Society, May, 24, 1869. (Linn. Soc. Proc. for 1869, p. xcii.*)

* The following references to passages of the kind referred to above as to the unreliability of craniographical evidence may be useful:—Geographisches, Jahrbuch, 1866, p. 481. Hyrtl, Topograph. Anatomie, i. p. 13. Henle, System. Anat. i. 198. Krause, Archiv für Anthropologie, ii. 1. Holder, *ibid.* See also His and Rütimeyer, and Eckers in their systematic works severally, the Crania Helvetica, and the Crania Germaniæ meridionalis.

The argument from identity of customs and practices to identity of race is liable to much the same objections and to much the same fallacies as is the argument from identity of cranial conformation. The case may be found admirably stated in Mr. Tylor's work on the "Early History of Mankind," p. 276, ed. 2, and I may say that the means of bringing the problem home to oneself may be found by a visit to any collection of flint implements. In such a collection as Mr. Tylor has pointed out, p. 205, we are very soon impressed with the marked uniformity which characterises these implements, whether modern or thousands of years old, whether found on this side of the world or the other. For example, a flint arrowhead which came into my hands a short time back, through the kindness of Lord Antrim, after having done duty in these iron times as a charm at the bottom of a water-tub for cattle in Ireland, was pointed out or at to me by a very distinguished Canadian naturalist, who was visiting Oxford the other day, as being closely similar to the weapons manufactured by the Canadian Indians. Now after such an experience one may do well to ask in Mr. Tylor's words ("Early History," p. 206):—

"How, then, is this remarkable uniformity to be explained? The principle that man does the same thing under the same circumstances will account for much, but it is very doubtful whether it can be stretched far enough to account for even the greater proportion of the facts in question. The other side of the argument is, of course, that resemblance is due to connection, and the truth is made up of the two, though in what proportions we do not know. It may be that, though the problem is too obscure to be worked out alone, the uniformity of development in different regions of the Stone age, may some day be successfully brought in with other lines of argument, based on deeplying agreements in culture which tend to centralise the early history of races of very unlike appearances, and living in widely distant ages and countries."

If the psychological identity of our species may explain the identity of certain customs, its physiological identity may explain certain others. Some of this latter class are of a curious kind, and relate not to matters of social or family, but to matters of purely personal and individual interest, concerning as they do the sensibility, and with it, all the other functions of the living body. Such customs are the wearing of labrets, or lip-rings, nose-rings, and if I may add it without offence, of certain other rings, inserted in the wide region supplied by the fifth or tri-facial nerve. A physiological explanation may lie at the base of these practices, which appear to put at the disposal of the persons who adopt them a perennial means for setting up an irritation, whence reflex consequences in the course of reflex nutrition and reflex secretion, as of gastric juice, may flow. A curious book was written, or at least published, on the subject of these practices, and others akin to them, in 1653, by Dr. John Bulwer, a benevolent doctor, who paid attention to the care of the deaf and dumb previously, I think it is stated, to Dr. Wallis, and who consequently, with proper pride, if this precedence really belongs to him, signs himself "J. B. cognomento Chirosophus." The title of the book is "Anthropometamorphosis; Man Transformed, or the Artificial Changeling." I was made acquainted with its existence by my friend, Mr. Tomlinson, of Worcester College, from the library of which Society I procured a copy for consultation; the book is not rare I think; but I think it is little known, it contains much that is curious, and it is, inasmuch as it was written more than 200 years ago, ὅτ' ἀκήρατος ἦν ἔτι λείμων, from some, though not from all points of view, the more valuable. It is, I apprehend, to some of these customs, as well as to others, that Zimmerman—not the author of the work on Solitude, but Zimmerman the zoologist—alludes in a rather amusing passage, which may be found in the third volume of his larger work on the Distribution of Species, and on Zoology (see p. 257). I speak of the passage as amusing, it is more than that, or I would not quote it; indeed you will not see that it is particularly amusing unless I tell you that volumes ii. and iii. are of date 1783, and are dedicated to his own father, whilst volume i., of date 1778, is dedicated to "His Most Serene Highness and Lord, Ferdinand Duke of Brunswick, my Most Gracious Lord." Its quality of amusingness depends upon these dates, and the speculations they set us to make as to how the Duke had offended the man of science in the interval between 1778 and 1783. It may be a warning to Serene Dukes how they

treat men of science. It runs thus :—"If you argue from similarity of customs and ceremonies to identity of origin of two tribes under comparison, you must first show that these customs are not such as would naturally tend to the amelioration of the conditions of the inhabitants in the two countries under consideration, and would probably therefore, or can naturally suggest themselves to each of the races in question. Or there may be customs founded on innate folly and stupidity, and thus, for your argument to be valid, you must show that of two peoples widely separated, each cannot by any chance come into its own country to adopt the like foolish and stupid customs. For whilst two wise heads are to make out, each independently of the other and contemporaneously, a wise discovery or invention, it is much more likely in the calculation of chances, and considering the much greater number of fools and blockheads ('Thoren und Dummköpfen') that in two countries widely apart, closely similar follies should be simultaneously invented. And then, if the inventing fool happens to be a man of influence and consideration, *which is, by the way, an exceedingly frequent coincidence*, both the nations are likely to adopt the same foolish practice, and the historian and antiquarian after the lapse of some centuries, is likely to draw from this coincidence the conclusion that the two nations both sprang from the same stock." Judge and speculate for yourselves how the spirit which breathes in this passage was excited, but note its scientific value too. We must not forget that it is possible, in thought, at least, to dissociate the psychological unity of man from his specific identity even; and that as regards identity of race, it is only reasonable to expect that when similar needs are pressing, similar means for meeting them are not unlikely to be devised independently by members of two tribes who have for ages been separated from their original stocks. The question to be asked is, does the contrivance about which we are speculating, combine or does it not combine in itself so large a number of converging adaptations, as to render it upon the calculation of chances, unlikely that it should have been independently invented? Yet this very obvious principle has been neglected, or Lindenschmidt would not have found it necessary to say, that by laying too much stress upon certain points of national identity in the stones used for the formation of cromlechs or dolmens, the Hünenvolk might be made out to have chosen to settle only in those parts of Germany where erratic blocks of granite or other such large stones could be found! (Archiv für Anthropologie, iii. p. 115, 1868.)

Sir John Lubbock's recently published work on "The Origin of Civilisation," may, I anticipate, cause the history and genealogy of manners and customs to enter largely into the composition of our lists of papers. There is no need for me, as the author of the book is here himself to speak, as announced, for himself, to occupy your time in recommending his work; but I may be allowed to say that the utility of such pursuits as those which Sir John Lubbock's book treats of, receives some little illustration from the fact that, as we learn from him and from Mr. Tylor, the human mind blunders and errs and deceives itself in these subjects in just the same way as it does in the kindred, though more immediately arising, pressing and important matters of social and political life. In these latter spheres of observation we are apt occasionally to mistake one of those intermittent reactions of opinion, produced as eddies are produced in a river, by the deposit of sand and mud at angles in its onward course, for a deliberate giving up of the principles upon which all previous progress has been dependent. The straws which float upon the surface of a backwater may be taken as proofs that the river is about to flow upwards, and a feeble oarsman in a light boat may be deceived for some moments by the backward drifting of his small craft. Now an analogous blunder is often made in matters of purely historical interest; and we may do well to learn from the experience thus cheaply earned. "The history of the human race has," says Sir J. Lubbock, p. 322, *l. c.*, "I feel satisfied, on the whole been one of progress; I do not of course mean to say that every race is necessarily advancing; on the contrary, most of the lower are almost stationary," but Sir John regards these as exceptional instances, and points out that if the past history of man had been one of deterioration, we have but a groundless expectation of future improvement, whilst on the other, if the past has been one of progress, we may fairly hope that the future will be so also.

Mr. Tylor's words are equally to the purpose, though as forming the end of a chapter merely, and that at the end of the book, they are less enthusiastic in tone. (P. 193, Tylor, "Early History of Mankind.") They run thus—

"To judge from experience, it would seem that the world, when it has once got a firmer grasp of new knowledge or a new art, is very loath to lose it altogether, especially when it relates to matters important to man in general, for the conduct of his daily life, and the satisfaction of his daily wants, things that come home to men's 'business and bosoms.' An inspection of the geographical distribution of art and knowledge among mankind seems to give some grounds for the belief that the history of the lower races, as of the higher, is not the history of a course of degeneration or even of equal oscillations to and fro, but of a movement, which, in spite of frequent stops and relapses, has on the whole been forward; that there has been from age to age a growth in man's power over nature, which no degrading influences have been able permanently to check."

I must not trespass into the province of the Botanist, but I should be glad to say that no easier method of learning how the natural history sciences can be made to bear upon the history of man, as a whole, can be devised than that furnished by the perusal of such memoirs as those of Unger's upon the plants used for food by man. The very heading and title of the paper I am specially referring to appears to me to have an ambiguity about it which, in itself, is not a little instructive. In that title, "Botanische Streifzüge auf dem Gebiete der Cultur-Geschichte," the latter word may be taken, I imagine, etymologically at least, to refer either to culture proper or to agriculture. At any rate, the paper itself may be read in the Sitzungsberichte of the Vienna Academy for 1857; it has, I suppose, superseded the interesting chapters in Link's "Urwelt und Alterthum," of date 1821; and it is not unlikely, I apprehend, to be itself, in its turn, superseded also.

Coming, in the third place, to Zoology, I suppose I shall be justified in saying that the largest issue which has been raised in the current year, an issue for the examination of the data for deciding which the two months of July and August which are just past, may have furnished persons now present with opportunities, is the question of the kinship of the Ascidians to the Vertebrata. There is or was nothing better established till the appearance of Kowalewsky's paper, now about four years ago, than the existence of a wide gulph between the two great divisions of the animal kingdom, the Vertebrata and the Invertebrata; nothing could be more revolutionary than the views which would obviously rise out of his facts, and within the present year these facts have been abundantly confirmed by Prof. Kupfer, whose very clearly written and beautifully illustrated paper has just appeared in the current number of Schultze's "Archiv für Microscopische Anatomie." Kupfer's researches have been carried on upon *Ascidia canina*, but they more than confirm the accuracy of what Kowalewsky had stated to take place in *Ascidia mammillata*, and which may be summed up briefly thus: In the larval Ascidian we have in its caudal appendages an axis skeleton clearly analogous, if not essentiallly homologous, to the chorda dorsalis of the vertebrate embryo, as consisting like it of rows of internally-placed cells, and giving insertion by its sheath to muscles. We have further the nervous system and the digestive taking up in such embryos much the same positions relatively to each other, and to this molluscan chorda dorsalis, that are taken up by the confessedly homologous system in the Vertebrata; we have the nervous system originating in the same fashion and closing up like the vertebrate myelen cephalon out of the early form of a lamellar furrow into that of a closed tube; we have finally the respiratory and digestive inlets holding the vertebrate relationship of continuity with, instead of the invertebrate of dislocation and separation from each other. Such are the facts; but I am not convinced that they will bear the interpretation that has been put upon them; though I must say the possession of this chorda dorsalis by the active locomotor larva of the Ascidian which one day settles down into such immobility, lends not a little probability to Mr. Herbert Spencer's view of the genesis of the segmented vertebral column in animals undoubtedly vertebrate. But on this view I should not be inconsistent with myself, inasmuch as, to waive other considerations, the chorda dorsalis in each case would be considered as an adaptive or teleological modification, not a sign of morphological kinship. Much perplexity may or must arise here, and whilst entertaining these views, I felt myself bound to examine myself strictly to find whether in not taking them up, I might not be giving way to that reactionary reluctance to accept new ideas which advancing years so frequently bring with them; but a recent paper, by Lacaze Duthiers, published in the *Comptes Rendus* for May 30, 1870, and translated in the Annals and Magazine of Natural History for July,

1870, would justify me, I think, in calling that reluctance by another name. For in that paper the renowned malacologist just mentioned has brought to light the fact that there is another sessile and solitary Ascidian, the *Molgula tubulosa*, which goes through no such tadpole-like stage, as had been supposed to be gone through by all Ascidians except the Salpæ, which is never active and never puts out the activity which is so remarkable in the other Ascidians, but settles down and remains sedentary immediately after it is set free from the egg capsula, neither enjoying a Wanderjahr *nor possessing a chorda dorsalis*. We are not surprised after this that M. Lacaze Duthiers observes that "although embryology may and must furnish valuable information by itself, it may also, in some cases, lead us into the gravest errors." Mr. Hancock, of Newcastle-upon-Tyne, has sent us a paper upon this subject, which will be read duly, and duly noted by us.

Leaving Malacology, which has not in the United Kingdom obtained the same hold as yet upon the public mind that it has on the Continent, where, like Entomology, there and here, it has a periodical or two devoted to the recording of the discoveries of its votaries, I have much pleasure in directing attention to two short papers by Siebold in the *Zeitschrift für wissenschaftliche Zoologie* (xx. 2, 1870), on Parthenogenesis in *Polistes gallica v. Diadema*, and on Pædogenesis in the *Strepsiptera*. In each of these short papers Siebold informs us that adequate room and time could not be given them in the Innsbruck meeting held just a year ago, or in the report of the meeting. It is to me a matter of difficulty to think what there could have been of greater value than those papers in a section of Wissenschaftliche Zoologie; it will be to all present a matter of congratulation to learn from the venerable professor's papers that he will shortly favour us with a new work on the subject of Parthenogenesis. A fresh instance of Parthenogenesis in Dipten, in *Chironomus*, has just been put upon record in the St. Petersburg Imperial Academy's memoirs (xv. 8, Jan. 13, 1870).

The subject of the Geographical Distribution of the various forms of vegetable and animal life over the surface of the globe, and in the various media, air, earth, water, fresh and salt, whether deep or shallow, has always been, and will always remain, one of the most interesting subsections of biology. It was the contemplation of a simple case of geographical distribution in the Galapagos Archipelago which brought the author of the "Origin of Species" face to face with the problem which the title of his work embodies; and it is impossible that sets of analogous and of more complicated facts,—many of which, be it recollected, such as the combination now being effected between our own Fauna and Flora and those of Australia and New Zealand, are patent to the observations of the least observing,—should not, since the appearance of that book, force the serious consideration of the explanation it offers upon the thoughts of all who think at all. The wonders of the Deep Sea Fauna will, I apprehend, form one, the Commensalism of Professor Van Beneden another, subject of discussion, and furnish an opportunity for receiving instruction to all of us. The one set of observations is a striking exemplification of the way in which organisms have become suited to inorganic environments, the other is an all but equally striking exemplification of the way in which organisms can fit and adapt themselves to each other. The current journals have,* as was their duty, made us acquainted with what has been done in both of these directions, and I am happy to say that in the case of the Deep Sea Explorations as in that of Parthenogenesis and Spontaneous Generation, a new work, giving a connected and general view of the entire subject, is announced for publication.

One instance of the large proportions of the questions which the facts of geographical distribution bear upon, is furnished to us in the address recently delivered before the Geological Society by its president, who is also our president, and who may have forgotten to refer to his own work (see NATURE, No. 24, 1870). Another may be found in the demonstration which Dr. Günther, contrary to our ordinarily taught doctrines, has given us (Zool. Soc. Trans. Vol. vi., pt. 7, 1868, p. 307) of the partial identity of the fish-faunas of the Atlantic and Pacific coasts of Central America; a third is furnished to us by Mr. Wallace's works *passim*.

It would be superfluous, after introducing even thus hurriedly to your notice so large a series of interesting and important subjects as being subjects with which we shall forthwith begin to deal in this Section, to say anything at length as to the advantages to be expected reasonably from the study of Biology. I may put its claims before you in a rough way by saying that I should be rejoiced indeed if when money comes to be granted by the Association for the following up the various lines of biological research upon which certain of its members are engaged, we could hope to obtain a one-hundredth, or I might say a thousandth part of the amount of money which has in the past year been lost to the State and to individuals through ignorance or disregard of biological laws now well established. I need say nothing of the suffering or death which anti-sanitary conditions entail, as surely as, though less palpably and rapidly than, a fire or a battle; and I might, if there were time for it, take my stand simply upon what is measurable by money. This I will not do, as it is less pleasant to speak of what has been lost than of that which has been or may be gained. And of this latter let me speak in a few words, and under two heads—the intellectual and the moral gains accruing from a study of the Natural History Sciences. As to the intellectual gains, the real psychologist and the true logician know very well that the discourse on method which comes from a man who is an actual investigator is worthy, even though it be but short and packed away in an Introduction or an Appendix, or though it cover but a couple of pages, like the "Regulæ Philosophandi" of Newton, more than whole columns of the "Sophistical Dialectic" of the ancient Schoolman and his modern followers. "If you wish your son to become a logician," said Johnson, "let him study Chillingworth"—meaning thereby that real vital knowledge of the arts and sciences can arise only out of the practice of reasoning; and as to the value of actual experimentation as a qualification for writing about method, Claude Bernard and Berthelot are, and I trust will long remain, living examples of what Descartes and Pascal, their fellow-countrymen, are illustrious departed examples. (See Janet, Revue de deux Mondes, tome lxii. p. 910, 1866.)

I pass on now to say a word on the working of natural science studies upon the faculty of attention, the faculty which has very often and very truly been spoken of as forming the connecting link between the intellectual and the moral elements of our immaterial nature. I am able to illustrate their beneficial working in producing carefulness and in enforcing perseverance, by a strong turning upon the use of, or rather upon the need for, a word. Von Baer, now the Nestor of biologists, after a long argumentation (Mem. Acad. Imp. Sci., St. Petersburg, 1859, p. 340) of the value which characterises his argumentations as to the affinities of certain Oceanic races, proceeds to consider how it is that certain of his predecessors in that sphere, or rather, in that hemisphere, as Mr. Wallace has taught us Oceania is very nearly, had so lamentably failed in attaining or coming anywhere near to the truth. This failure is ascribed to something which he calls "Ungenirtheit," a word which you will not find in a German dictionary, the thing itself not being, Von Baer says, German either. I am happy not to be able to find an exact equivalent for this word in any single English vocable, the opposite quality shows itself in facing conscientiously "the drudgery of details, without which drudgery," Dr. Temple tells us (Nine Schools Commission Report, vol. ii., p. 311), "nothing worth doing was ever yet done." Mr. Mill, I would add, speaks to the same effect, and even more appositely, as far as our purpose and our vocations are concerned, in his wise Inaugural Address at St. Andrews, p. 50. For the utter incompatibility of an *δραλαίπωρος ζήτησις*,—those two words give a Thucydidean rendering of "*Ungenirtheit*,"—with the successful investigation of natural problems, I would refer any man of thought, even though he be not a biologist, to a consideration of the way in which problems, as simple at first sight as the question of the feeding or non-feeding of the salmon in fresh water (see Dr. McIntosh, Linn. Soc. Proc., vii., p. 148), or that of the agencies whereby certain molluscs and annelids bore their way into wood, clay, or rocks must be investigated. It is easy to gather from such a consideration how severe are the requirements made by natural science investigations upon the liveliness and continuousness of our faculty of attention.

I shall speak of but one of the many purely moral benefits which may be reasonably regarded either as the fruit of a devotion to or as a preliminary to success in natural science. Of this I will speak in the words of Helmholtz, taking those words from a report of them as spoken at the meeting of the German Association for the Advancement of Science, which was held last year at Innsbruck. There Professor Helmholtz, in speaking of the distinctive characteristics of German scientific men, and of their truthfulness in particular, is reported to have used the following words:—

* See NATURE, No. 39, July 28, 1870, and "Royal Society's Proceedings," Aug. 1870, for Deep Sea Explorations, and *Academy*, Sept. 10, 1870, for Commensalism.

"Es hat diesen Vorzug auch wesentlich zu verdanken der *Sittenstrenge* und *der uneigennutzige Begeisterung* welche die Männer der Wissenschaft beherrscht und beseelt hat, und welche sie nicht gekehrt hat an aussere Vortheile und gesellschaftliche Meinungen." These words are, I think, to the effect that the characteristics in question are in reality to be ascribed to the *severe simplicity of manners* and *to the absence of a spirit of self-seeking*, which form the guiding and inspiring principles of their men of science, and prevent them from giving themselves up to the pursuit of mere worldly advantages, and from paying undue homage to the prejudices of society. I think *Sittenstrenge* may be considered as more or less adequately rendered by the words *severe simplicity of manners*; at any rate, as things are known by their opposites, let me say that it is the exact contradictory of that "*profound idleness and luxuriousness*" which, we are told by an excellent authority (the Rev. Mark Pattison, "Suggestions on Academic Organisation," p. 241),—for whose accuracy I would vouch in this matter were there any need so to do,—"*have corrupted the nature*" of a large class of young men amongst ourselves, whilst *the absence of a spirit of self-seeking* is, in its turn, the contradictory of a certain character which Mr. Mill (*l.c.*, p. 90), has said to be one of the commonest amongst us adults, and to which Mr. Matthew Arnold has assigned the very convenient epithet of "Philistine." Investigation as to whether these undesirable tendencies are really becoming more rife amongst us, might be carried on with advantage in a place such as this, in the way of inquiries addressed to colonists returning home after a successful sojourn abroad. Such persons are able to note differences without prejudice, and, *ex hypothesi*, with unjaundiced eyes, which we are apt to overlook, as they may have grown up gradually and slowly. But, perhaps, researches of this kind are not quite precisely the particular kind of investigation with which we should busy ourselves; neither would the leaders of fashion, the persons with whom all the responsibility for this illimitable mischief rests, be very likely to listen to any statistics of ours, their ears being filled with very different sounds from any that, as I hope, will ever come from Section D. Whether men of science in England are more or less amenable to blame in this matter than the rest of their countrymen, it does not become us to say; but it does become and concern us to recollect that we have particular and special reasons, and those not far to seek, nor dependent on authority alone, for believing and acting upon the belief that real success in our course of life is incompatible with a spirit of self-seeking and with habits of even refined self-indulgence.

Department of Ethnology and Anthropology.—John Evans, F.R.S., in the chair.

Before commencing the business of the department, Mr. Evans offered a few remarks as to the subjects that properly came within the province of the department—the present condition of knowledge of those subjects, and the methods at command for increasing that knowledge. The subjects which may with propriety be brought under the consideration of the Department may approximately be defined as:—(1) all that relates to the antiquity of man, or the origin of the various races of mankind; (2) all that illustrates the progress and development of human civilisation; and (3) all that concerns the condition of the less civilised portions of the human race, even if not immediately connected with any general question of its origin or progress. The President then proceeded to show what an enormous field these subjects embrace, how much there is still to learn in the means of investigating them, notwithstanding the efforts of the numerous labourers, who have now for many years been employed in this field of research, and concluded a very interesting address by expressing his confidence that nothing would be said calculated to injure the feelings of any who, like ourselves, are in pursuit of truth, and that all will bear in mind how difficult it is to take in the whole of any single truth at one view, and how, of its many sides, two contending parties may each be seeing one only, and that possibly not the most important.

On the Principal Geological Changes which have occurred in Europe since the Appearance of Man.—Professor P. Martin Duncan, F.R.S., F.G.S., &c.

This short communication does not pretend to offer any new facts or opinions; but I trust that it will be found to be a truthful representation of the results of the more important labours of men who have laboured so sedulously and conscientiously on the subject of the antiquity of man. As such, I hope the paper will be of interest to the associates of the Section, although I cannot anticipate that it will yield any satisfaction to my more learned colleagues.

Most of the geological changes which have occurred since man first appeared in Europe are estimated and asserted in consequence of the results of direct observation upon the succession of phenomena which belong to the debatable ground between what is usually termed physical geography and geology. Some, however, are dependent upon the arguments which are brought forward by naturalists respecting the limitation and separation of parts of great natural history provinces.

For instance, if the remains of man or his early works are found in sediments high up on the sides of valleys, the sediments having been produced by the river when it flowed far above its present level, the vertical distance between the remains and the existing water-level affords evidence of geological or physico-geographical changes. And if a vast number of bones belonging to extinct and existing species of large animals, such as elephants and rhinoceroses, hyænæ and lions, are found upon islands the area of which could not possibly have sustained and nourished the mammalia during life, the separation of the areas from the nearest continent is inferred, especially if the species are still existing there, or if their remains abound there.

It is necessary to premise that no trace of man has been found associated with any deposits which are formed during the Glacial Period in Northern Europe. The very nature of them would prevent such a discovery.

The loftiest mountains of Europe, such as the Alps, Pyrenees, Ardennes, and Vosges, underwent a great grinding down during the Glacial Period; and when it was over the results of this wear and tear were scattered far and wide all around them in the form of wash-down, gravel, and more or less angular stones. In a general view, this gravel, the result of the first glacialisation, is of the same geological age as the drift gravel of England north of the Thames, and of Northern Germany, which is called glacial drift, and which, as I have just observed, is older than the earliest traces of man in Europe. The earliest remains of man and his works, and of the beasts associated with him and hunted by him, rest upon these deposits, and are therefore later in time.

It would appear that man followed up the retreating ice of the north of Europe, for the remains of his works are found high up in many British valleys, which must then have begun to be formed by the natural drainage out of the deposits of the Glacial Period.

Now, after a time, there was another period of mountain glacialisation; and the glaciers of the Alps and Pyrenees, especially, extended far into the districts below them.

The grinding down of the mountain sides during this second glacialisation produced enormous quantities of mud and gravel; and when the glaciers retreated this detritus was washed far and wide over the plains. This deposit is constantly found to cover the remains of man and his works, and is therefore later in time.

The second glacialisation, and the dispersal of the wash-down, appears to form a rude line of separation between the Palæolithic Period, when man used rude stone weapons, and the Neolithic Period, when smooth or polished implements were made by him; and they also, in a general sense, mark the time when the great mammalia, the early prey of man, disappeared from the northern and western parts of Europe.

The following are the principal geological changes which occurred after the appearance of man in Europe:—

1. The subsidence of an area of land which connected Sicily with Crete and Northern Africa north of the Sahara.

2. The formation of a volcanic tufa on the hills bordering the present valleys of the Tiber and its tributaries; the excavation of those valleys by the river and its streams; the last eruption of the volcanoes of Latium, and their permanent extinction. The space included in the Roman territory has received its contour, and vast tracts near the coast have been worn away.

3. The formation of valleys in the Alpine detritus, which covered up large tracts of Northern Italy, and the re-excavation of old valleys, which had been more or less filled with the detritus. This great gravel was the wash-down of the wear and tear of the first extension of the south Alpine glaciers, and was, in a general sense, contemporaneous with the upper glacial drift of Northern Europe. It was deposited before man, as traced by his relics and works, lived in South Europe.

The dispersal of vast depths of silt and gravel over the plains into the valleys and far up the hill sides of the Sardinian States and Lombardo-Venetia, south, and to a certain extent south-east, of the Alps; the result of the wash-down of the wear and tear of the second extension of the Alpine glaciers; the forma-

tion of valleys in this gravel or silt, and the production of such heights as those which bound such plateaux as Rivoli.

4. Considerable local alterations in the relative level of land and sea along the west Neapolitan coast.

5. The formation of the straits of Gibraltar.

6. The excavation of such valleys as that of the Manganores in Central Spain, the formation of gravels containing flint implements and mammalian bones near Madrid, and therefore far beyond the influence of marine action.

7. The wearing down of many of the valleys to the north of the Pyrenees below the level of such lower mammaliferous caves as those in the neighbourhood of Tarascon; the dispersion of the results of the wear and tear of the second extension of the Pyrenean glaciers, and the filling up of the old valleys with it; the re-excavation of the valleys, and the carrying down of their silt or loess to the plains; the formation of streams and watercourses through this deposit.

8. The formation of certain valleys in the Perigord by streams to a certain extent, but principally by the gradual effects of rain, heat, frost, and other meteorological actions.

9. The excavation of the valleys of North and Eastern France, and the denudation and retrogression of their watersheds.

10. The dispersion of Alpine rocks, gravels, and rocks to the north of the Alps, produced by the wear and tear of the great glacialisation, which was, in a general sense, contemporaneous with the first extension of the glaciers south of the Alps, and the first extension of the Pyrenean glaciers, occurred before man appeared in Europe. After his first wanderings and huntings he left his remains above this old Alpine detritus. Then the valleys in the carboniferous limestone of Belgium were worn down ninety yards or more by rain and rivers, and the bones of the extinct mammalia and rude stone implements and bones of man were washed into caves with the gravel.

After the retirement of the glaciers, subsequent to this second extension, the wash-down of the Alps, Vosges, and Ardennes was spread over the older gravel. It filled up the valleys, and extended with a thickness varying from a few yards to a thousand feet and more, all down and over what is now the valley of the Rhine, Holland, and Belgium. The loess thus formed was then washed out of the valleys, was cut into by rivers, and has been worn down ever since.

11. The separation of the coasts of France and England about Dover and Calais.

12. The excavation of nearly all the valleys in the district east of a line drawn from King's Lynn to Portland, the denudation of their watersheds, and retrogression of the river sources.

13. The denudation of the valley of the Weald of Kent.

14. The separation of the Isle of Wight from the main land.

15. The formation of a great part of the Bristol Channel.

16. The upheaval of many sea beaches, and the general destruction of forest land on the south and west of England; the formation of many peats.

17. Enormous destruction of the sea coast.

18. A sufficient upheaval of the Scandinavian peninsula and Denmark, to produce such a restriction of the outlet as has determined a change in the marine fauna of the Baltic.

19. A slow upheaval of large areas appears to have accompanied the excavation of the valleys upon them, and a subsidence of equally large districts appears to have accompanied the recession of the second glaciers; probably another upheaval followed.

20. The uprise of the Desert of Sahara in Africa after the second extension of the Alpine glaciers.

Section E.—Geographical Science

On Atmospheric Currents.—Mr. J. K. Laughton. In examining into the geographical distribution of winds, we must bear in mind that well-attested and careful observation is the only satisfactory basis, and that descriptions founded on theoretical opinions are of no value whatever. If we refer Hadley's Theory of the Trade Winds to this test, we find, *in the first place*, that the effect of heat in producing wind is not quite such as has been represented. Experimentally, heat does not produce a blast, unless the space between the heat and cold air be very confined, as is roughly shown by holding a newspaper before the fire. Geographically, heat does not cause a wind towards any of the principal areas of greatest temperature; either towards the Great African Desert, the Desert of Arabia, or of Australia, towards the Red Sea or the Persian Gulf, or even, when carefully traced, towards the Great Prairie of North America. *In the second place*, we find that the effect attributed to the rotation of the earth is not consistent with numerous observed facts, such as the S.E. wind in the Gulf of Mexico, the N.W. wind on the coast of North Africa, the N.W. gales in the North Atlantic, the S.W. wind on the south coast of Australia, and very many others; and, indeed, the idea appears to have originated in a temporary forgetfulness of the power of friction, which in the case of air and all fluids is very intense.

Winds, which, in accordance with Hadley's theory, have been very generally divided into *polar* and *equatorial*, seem more naturally to divide themselves into *easterly* and *westerly*; and it is this division which has, from the days of Columbus, been adopted by really practical men, to whom the winds were matters of fact, not of mere theory; but the trade-winds—having attracted early notice by their very great steadiness and regularity—have always been considered as the direct manifestations of the first cause, whatever it might be, of the great atmospheric movements; the westerly winds, which were not discovered till much later, having been considered as secondary and comparatively of little importance. But, as our experience grows larger, we learn that the westerly winds have an extent and a strength and a rude vigour incompatible with the idea of their secondary nature. Whether in the northern or southern hemisphere, they are exceedingly violent and boisterous; and, without considering the Arctic and Antarctic regions, concerning which we have not sufficient evidence, they extend from 60° N. to 60° S., interrupted only by the trade-winds, which blow over an area large indeed, but small *in comparison* with that on which they intrude. The trade-winds are also of very limited height, whilst above them the westerly winds blow as strongly as they do in the temperate zones, where they reach into the upper strata of the atmosphere as far as we have any knowledge. We are thus led towards the conclusion that the westerly winds are really the primary winds, far extending and boisterous; whilst the quiet equable trade-winds—of very limited volume—are reflex streams of air caused by the impact of the great westerly winds on the continental barriers, whether against mountain ranges or the more sluggish air which lies over the land. The Atlantic Ocean affords us the most familiar illustrations of this; where we see the main westerly stream dividing on about the parallel of 45° N., and turning north, as a south-westerly wind on our coasts, and the coast of Norway; or south, as a north-west and northerly wind on the coast of Portugal, and a north-easterly on the coast of Africa; whilst the rest of it forces its way onward, a westerly wind over Northern Europe and Asia, or a northerly deflection in the several basins of the Mediterranean. On the other hand, on the extreme west, the westerly wind continually dragging away the air from the eastern side of the Rocky Mountains, causes such a tendency towards a vacuum, that the air from the south and north is induced towards it, and the wind over Western America rules from the south or north, according as the season throws the axial line of the temperate zone to the north or south of its mean position. Examining at great length into the various local winds and irregularities in the different parts of the world, we arrive at the conclusion that the whole atmosphere has a continued tendency to move from west to east, and does so more when it is not interrupted. The interruptions are of two kinds: one occasional and irregular, being caused by fluctuations in the hydrostatic condition of the air, the other permanent or seasonal and regular, caused by the pressure of lines of coast and mountain ranges.

It is impossible to say definitely why the atmosphere should have this prevailing motion; but if the cause is neither heat nor the influence of the earth's rotation, nor any agency which we can detect at work on the earth, we are driven almost insensibly to the belief that it must be the result of celestial attraction; and the fact that the barometer shows no trace of any noteworthy rise or fall, as of an atmospheric tide, suggests that the atmospheric currents, which must necessarily be formed by the action of such an intense disturbing force, do not in any way clash, but flow uninterruptedly onwards in one certain direction, either towards the east or towards the west. All observation shows us that there is not a permanent current towards the west, but that there is one towards the east; and although we are unable at present to master all the details of the manner of the motion, the evidence of geographical fact, combined with that of astronomical possibility, justifies us in inclining towards the belief that the motive force for which we are seeking is really the disturbing force of the attraction of the heavenly bodies.

SECTION G.—*Mechanical Science.*—President, Mr. Charles B. Vignoles, F.R.S., Pres. C.E.

In his introductory address, bearing chiefly on railway development and the defence of the country, the President remarked that that day, the 15th of September, was the anniversary of an event in which Liverpool played a most important part forty years ago. The opening of the Liverpool and Manchester Railway was entirely owing to the high public spirit shown by the merchants of Liverpool. At that time the ways and means of communication were so completely crippled that the trade of Liverpool would have become paralysed, had not better, speedier, and cheaper means of communication been created, and especially by the opening of the railway connecting Liverpool and Manchester. In this great work Liverpool took the lead; and it was particularly owing to the zeal of one of Liverpool's most distinguished citizens, the late Mr. Henry Booth, who was the original secretary of the railway company, and to the energy and scientific knowledge which he brought to bear upon the question of railways, that attention was paid to the improvements of the steam-engine, which was now performing such wonders both at home and abroad. He (the President) thought the people of Liverpool had not shown themselves sufficiently grateful to the memory of Mr. Henry Booth in allowing his name to lapse, as it were, from public recollection; but he was glad to have been invited to a meeting held in Liverpool a short time ago, at which a subscription was inaugurated for the raising of a statue to that eminent man and successful worker in mechanical science. The subscription list needed only a little addition to complete this most desirable object. Liverpool was peculiarly appropriate for this meeting of the British Association, for that Association and the railway system might be said to have had their birth in the same year, both having originated just forty years ago. He had himself taken part in laying down the Liverpool and Manchester Railway, and felt a special interest in this anniversary. The President then went on to mention the principal subjects which would be discussed in that Section. He next referred to the position of this country in regard to the means and preparedness for military defence, and as to the military service of England. He might, perhaps, be disabusing the minds of many persons who had supposed that the Government of this country was not prepared or was not alive to the necessity of creating the best means of internal communication, in the event of war or invasion of the country, when he mentioned to them that for several years past the military authorities had been in constant communication with the chief engineers of the country, and had formed deliberate arrangements, by which, in the event of such a casualty as a military invasion of England, within forty-eight hours the military forces of the whole country, say 100,000 men, might be brought down upon any given point that might be attacked. Therefore they might feel reassured as to the position of England in case of an invasion. As an old soldier himself, he had, at the request of the Government, treated this question, and had shown completely that within twelve hours of the alarm of invasion at any given point, the rolling stock of the railways of the country could be applied for concentrating all the military resources of the country upon the point threatened. He had stated in Liverpool forty-one years ago that the institution of railways would have this very result; and it had been practically exemplified in the war that was raging on the Continent. On Tuesday, he might mention, papers would be read upon the construction and stability of ships by some of the most distinguished men in the country, bearing upon the most unfortunate accident that had occurred to the ironclad steamship *Captain*, to the causes of which the attention of the whole scientific world was now being directed.

REPORT OF THE COMMITTEE ON BOILER EXPLOSIONS

Mr. Lavington E. Fletcher, C.E., reporter. The other members of the Committee were Sir William Fairbairn, Bart., C.E., F.R.S., LL.D., &c.; Sir Joseph Whitworth, Bart., C.E., F.R.S.; John Penn, C.E., F.R.S.; Frederick J. Bramwell, C.E.; Hugh Mason, Samuel Rigby, Thomas Schofield, Charles F. Beyer, C.E.; and Thomas Webster, Q.C., F.R.S.

In concluding their report, the committee stated that they decidedly incline to the plan of enforcing inspection directly by law rather than indirectly by penalty. They are not without apprehensions that, however ingeniously the principle of joint-stock insurance might be surrounded with a series of checks and counter-checks, yet that it would lead to inspection being cut down to the lowest possible point. On the other hand, were the inspection enforced by law, and nationally administered either by a central steam board or by a series of district ones, they consider that a far more generous system would be secured. The steam boards, uninfluenced either by private or local interests, or by the desire to accumulate profits, would take altogether higher ground, and inspect, not simply in their own interests, and just sufficiently to narrowly escape explosion, but with a view to assist steam users, disseminate practical information on the making and management of boilers, and promote progress. These objects would be altogether foreign to competing joint-stock insurance companies. The committee hold the view that, had coroners' verdicts been as satisfactory as they might have been, boiler explosions would not have been as numerous as they now are. With the additional experience of another year they feel compelled to take one other step in advance, and they have come to the conclusion that the time has arrived when the Government should enforce the periodical inspection of all steam boilers. They are convinced that explosions might be, and ought to be, prevented; that competent inspection is adequate for this purpose, and that any well-organised system of inspection extended throughout the entire country would practically extinguish boiler explosions, and save the greater part of the seventy-five lives now annually sacrificed thereby.

A paper on the same subject was read by Mr. E. B. Marten, C.E., and the discussion was taken upon both communications. The speakers were Sir William Fairbairn, Mr. Siemens, Sir Joseph Whitworth, Messrs. Hawksley, Bramwell, Rigby, Longridge, Gray, Maller, Sir William Armstrong, and the President. In summing up, the President remarked that many accidents were attributable to the dishonest construction of boilers. English habits seemed to kick against anything like Government interference, but such accidents as had arisen from boiler explosions should be put an end to as forcibly as possible—like stamping out the smallpox or the cattle plague, notwithstanding vulgar prejudices—if necessary, by an iron hand. The Government should pass a law making the inspection of boilers compulsory.

On Mechanical Stoking.—Messrs. James Smith and J. and T. Vicars, Liverpool. The paper is too long to reproduce here, but we may mention, in reference to the method described in it, that Mr. L. E. Fletcher, C.E., of the Steam Users' Association, remarked that he had witnessed some very carefully-conducted trials with the apparatus as against very careful hand-firing, and that he could testify that the furnace was perfectly smokeless, and in every respect attained good results.

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ERRATA.—Page 391, col. 2, the asterisk referring to first foot-note should have been placed after the word "feet," line 33—Page 393, col. 2, line 5 from bottom, for "Cyclus" read "Cyclas"; line 2 from bottom, for "Surg." read "Surv." Page 408, col. 2, Contents, for "G. D. De Rance" read "C. E. De Rance."—Page 423, col. 2, line 34 from bottom, for "this" read "their"; line 6 from bottom, for "Gaskell's" read "Quekett's"—Page 424, col. 1, line 30 from bottom, for "*gradatium*" read "*gradatim*"; col. 2, line 4, for "Loca" read "Local"; line 6, for "this" read "the"; line 5 from bottom, for "Darwin's" read "Damon's"—Page 425, col. 1, line 5, for "conjuvat" read "conjurat"; line 8 from bottom, for "*Ajax* or" read "*Ajax* of"; col. 2, line 28, for "which" read "who"—Page 426, col. 1, line 13, for "war" read "woe"; line 34 from bottom, for "knotgrass, cowgrass" read "knotgrass s. cowgrass"; line 24 from bottom, for "*porcena*" read "*porcina*"—Page 427, col. 1, line 1, for "Bichamp" read "Béchamp"; line 17 from bottom, for "may come when" read "may, when"; col. 2, line 35, for "inhalistic" read "vitalistic"; line 36, for "useful" read "vital"; line 37, for "involve" read "resolve"; line 39, for "ordered" read "resolved."